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THE ÆSTHETIC PRINCIPLE IN COMEDY[1]

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Although it is fashionable nowadays to praise the 'sense of humor,' there is a traditional rôle for critics of art which consists in deploring and cavilling at the human love of laughter. To pursue the laughable is almost invariably, according to this tradition, to sacrifice the high for the low, the excellent for the perverse. Supremacy, in art as in all walks of life, is taken to be isolated and sorrowful; beauty's majesty must wear the buskin. The marriage of æsthetic excellence with tragedy is indeed not only a legend of the elect, it is a commonplace of popular culture. The acclaimed art of our human inheritance has the power to awaken sadness; the acclaimed masters are masters of the mournful note,—Æschylus, Euripides, Michael Angelo, Dante, Shakespeare, Goethe, and who else you will,—their best is their most funereal. Nevertheless increase in humane quality may be fairly gauged by gain in the scope of laughter. While it is untrue that savages are without a sense of humor, it is true that their derision has a narrow range and fixes itself upon the more fleshly if profounder aspects of the common lot,—upon the pursuit and capture of food, upon the business of marriage and childbearing, upon the enhancement and glory of the self. These great central interests are, no doubt, the piteous matter of amusement for civilization also, and our populace has hardly attained a wide vision of the comedy in the residual world; but it nevertheless has such a vision, and is appreciative of

[1]This paper is part of the third chapter in a book—"Beauty and Use: Outlines of a Pragmatic Philosophy of Art"—now in preparation.

the range of the comic through institutions and ideas, through the sacred and the lofty, as well as through the natural and the instinctive. Civilized mankind has gained on the unsophisticated in so far as it can laugh and command where the savage trembles and is afraid, while the greatest master of life seems to be he who, like Democritus, understanding the world's nature, laughs at its manners.

A profound and vital reason exists for this human love of the comic, for this increasing power to find and to place it, for the fact that the majority pursue it, if not more eagerly, as eagerly as they pursue beauty; for the fact that the cult of the 'sense of humor' has perhaps more shrines and a greater body of worshippers than the institutional cult of beauty. The love of beauty is the love of happiness; its possession in the æsthetic experience is the joy of successful self-conservation. Beauty is the directly-felt goodness of the environment. The environment arrests you as you plod or scurry in your daily routine; it holds you, brings all the faculties of your organic self to play upon it instantaneously, integrates them, sums them, until you attain whatever enduring optimum of value the environment offers. Beauty is this optimum of value, this realized entelechy of harmonious and instant interplay in adaptation of your whole self with that particular environment. Now the behavior of the comic is much the same. It, too, comes upon you suddenly during the affair of living; it, too, arrests and deploys your life, compelling it to take hold of the comic essence it offers you, and to it also you are adapted in the instant, harmoniously, completely, directly. On the other hand, there are certain well-marked differences between the experience of the comic and the experience of the beautiful. The former seems more complex, both with regard to your own state and the condition of the object. Their elements are harder to grasp and more difficult to hold. For yourself,—you smile, at the very least; ordinarily, you laugh. For the object, there is something that corresponds to your own condition,—an uncertainty, a movement, in character and in form.

Consider these differences more closely, of course only so far as they are ordinary, healthy and normal; the transnormal marvels of laughter are not our affair. In your own attitude the most striking point is the fact that it is an *action;* this smiling and laughing is something *doing,* and it is a doing which you love, which you prefer and persist in. To laugh is a privilege and a delight; and to be laughed at is, significantly, a degradation and a pain. It is not so with beauty; to be beautiful is even more agreeable than to enjoy beauty. This irreversible direction of laughter, well-exemplified in its

contagion, becomes still more significant when we observe its details. There is first the smile; the corners of the upper lip are drawn up, the canines and the incisors, the renders and the cutters, are laid bare, wrinkles form under the eyes, which narrow and brighten; there is a slight heightening of the respiration. There is also, perhaps, a barely perceptible outward movement of the hands. Very little is needed to pass from this smile, which may of course be reduced to mere upward twitch of the lips or a mere wrinkling of the eyes, to the quiet, audible laugh,—just a deep, not frequently noticed inspiration, then expiration in short, quick puffs, or chuckles or gurgles, accompanied by more noticeable wider expansive movements of hands and legs. If the laughter grows farther, is less restrained, then the head is thrown back as when swallowing a very agreeable morsel, the alternating inspiratory and expiratory processes grow more and more obvious and prolonged, the explosion of sounds louder, of varying pitch; the eyes are narrowed tb a frown, tears come, the limbs are thrown far out, or the body sways back and forth rhythmically, in wider and wider arcs, the hands are extended and slapped together. If the occasion or witness of the laughter is a person, he may be slapped on the back, poked in the ribs, or even embraced. Withal the blood-vessels are dilated, the blood comes faster through the system, more oxygen reaches it. In a word, the general vitality is heightened, the basis of being extended. The whole phenomenon of laughter seems expansive, enlarging, vitalizing; all its movements appear as if intended to embrace and absorb their occasion.

And that occasion,—supplied by nature, created and modified by art? However it occurs, it must be given whole before it can evoke its laughing response. The maker of an unpremeditated joke does not laugh when he makes it, he cannot; he laughs like his auditors, after he has heard it, after he has taken in the comic substance for what it is. And the apparently frequent anticipatory laughter of the auditor, that is in no sense directed upon what is not yet but will be; it is directed upon a content already offered and found comic. The essential condition of laughter,—paradoxical, commonplace as it may be,—is the actual apprehension of the concretely present laughable.

This, both in nature and in art, has many forms, widely diverse, disparate and difficult of comprehension under a single rubric. In nature there is earliest of all, the eleemosynary 'laugh' of the well-fed, replete, resting child repeating in its contentment the pleasurable movements of sucking, so much like laughter, so essentially a smile. The object which especially evokes it is said to be the rise of the food in the gullet,

so that the action would be like chewing a vicarious cud. But this is the mere beginning of laughter, and its occasion is problematic. A far more certain occasion is tickling. Now tickling seems to be a pleasure both sought and dreaded. The child's responsive actions to the tickling stimulus are partly defensive, opposing, mainly expansive and embracing. It seems to contain two elements uncertainly mixed, alternating, undirected, carrying both menace and safety, with the element of safety predominating. Under favorable conditions the whole or any portion of the body responds to it. An expected contact of an unknown and thus far discomforting stimulus turns out to be a contact of pleasure and delight. There is an essential conflict and titillation between two diverse elements of which the personality-feeling, whatever that be, finally finds itself free and master.

The daily life offers many instances which are determinable as complications of the characteristic contents of tickling. The laughter which follows fear, emotional or intellectual tension, is such. So when a child laughs after having been frightened by a dog, a woman after having heard bad news or on the shock of some vision or encounter, the terrifying object has seized on the mind, disorganized it, upset its equilibrium, emotionally or otherwise, is a menace to its proper character. When for whatever reason, it lapses, when this process dies down, when the organism has, with temporary or permanent success, resisted and vanquished its enemy, the engaged energies are released, the disturbed equilibrium is restored, the organism is again in possession of itself, and in a single instant or a longer period, it does not matter, apprehends the whole of the lapsed situation with the failure of its enemy and laughs, spontaneously, instinctively. Literature affords many instances of the same thing,—the typical laughter of mad Ophelia, Hamlet's curious ironical play with the ghost:

"Well said, old mole. Canst work i' the ground so fast? A worthy pioneer"

are instances. The preceding experience seems, so to speak, to break off and to constitute an object in which an element formerly a menace or a terror, exalted above the protagonist, has been thrown in the dust and made of low degree.

The laughter of sheer health might seem to be almost antithetical to this,—frequent, free, easy, evoked by the most trifling instances,—the sight of food, of friends, of strangers, the most ordinary events and actions. But it is not intrinsically different. Joyous though this laughter is, it is most prone to break out upon sudden stimuli, the overflowing energy of health seizes its unsuspecting object, is master of it *ab initio*,

and perverts its natural and proper relations to the world in which it belongs. The apparently meaningless laughter of sturdy children is such an action, the laughter of savages who are sufficiently familiar with strangers no longer to fear them, the very confident laughter of crowds, the careless laughter of people in power. Health, which is self-assured, stable, optimistic, finds everything grist for its mill of laughter, that is in the least different from it,—that is less stable than it. Health is literally wholeness, a self-sufficiency and completeness. The laughable, in so far as it is like tickling, is conversely not sufficient in itself, nor complete nor balanced nor stable. It seems less than health, and at its mercy.

This is perhaps nowhere so apparent as in play and make-believe. Those who have watched children at it must remember pleasantly how, wherever this play is collective, it is punctuated by continual bursts of laughter, sometimes accompanied by screams of it. Those who have questioned children about the persons and objects of their simulation, the characters they and their playthings assume, will not fail to recognize how deep a sense of the stability and reality of their customary environment children really have, and how rare are illusions on their part concerning the status of their fictions. For most of them, even the youngest, there is nothing magical or strange even in the most mechanical toys. Their sense of mechanism, indeed, seems stronger than their sense of mystery, of personality, of faerie. They do with their make-believes what suits their convenience; and what essentially suits their convenience is the domination and supremacy of the person they are. If they "play school" they insist either on being teacher, or on being victoriously troublesome pupils; if they personate characters, they insist on being the gloating all-vanquishing champion; Tom Sawyer as bold Robin Hood must kill the sheriff of Nottingham, but then Bill Harper, who was the dead sheriff of Nottingham, must also subdue Robin Hood. He cannot endure to be dead, even imaginatively. The laughter of play, then, apart from the physiological elements which like tickling depend upon titillation of expectancies, of physical contacts aimed and missed, of purposes crossed and frustrated, is a laughter directed upon an immediately apprehended difference between fiction and reality; and is the sense of vital power of control over both. In that more malicious form of play known as teasing, this becomes still more evident,—for teasing is play on the edge of earnest, pleasure on the edge of pain. Both the teaser and the teased laugh,—the teaser because he sees the contrast between the expectations of his victim and the character of his own intentions, because in that respect his

victim is at his mercy; the teased, because he recognizes the deceitful nature of his ostensible danger, because in his alarm at its on-coming he can still take it for what it is and so cause it to fall short of its intent. If he succeeds in doing so utterly, he turns the tables on his persecutor who thereby himself becomes the victim; if he fails in doing so, he becomes angered and the situation turns from fun to gravity. And with what ease, so often! A wink, a look, a word, may serve to turn a play of wit into a quarrel, a friendly game at cross purposes into a struggle for life.

Laughter, indeed, is intimately and often the clearest expression of victory in such vital struggles. The shouting laughter of partisans at great spectacular games in which their sides are successful, the wide, expansive, absorbing movements of throwing arms and limbs far out into the air, swinging hats and dancing attest this relation. It is evinced in the traditional report of the sucessful prize-fighter who toward the end of his combat 'comes up smiling.' Usage indicates it in 'the self-confident smile' attributed to any one who is master of an art or of a situation. Popular wisdom expresses it in the proverb 'He laughs best who laughs last.' Victory in combat of any sort whatsoever may be accompanied by laughter,—when the tension of the combat is relaxed, when the mind erects itself and surveys the event and the prostrate enemy. The laughter does not occur during the battle; during the battle there is silence, grim absorption in the business at hand. The occasion of laughter is not the combat, but the fallen in combat, the vanquished enemy, the mighty laid low, the peer reduced, the apparent strength unmasked and laid bare for the weakness it really is, while the victor remains firm, unshaken and laughing in his might.

The denudation or exposure of things, the inversion of appearance by reality before a witness whose own 'reality' remains firm, whose seeming and being are by contrast one, is indeed the basis, together with this envisagement of the defeated enemy, of the most universal matter of laughter nature supplies,—the laughter of sex. Fully nine-tenths of the witticisms of daily life, and more than half the wit of literature plays on sex. Sex is laughable because social life requires that it be hidden, set aside, submerged; while the natural endowment of man impels the instinct to raise its head out of the darkness, to peer into the light of day. This traditional throwing-off of linguistic, sartorial or customary convention causes laughter. The peasant and the boor, by use of language, do so directly,—the mere mention of matters allied to the reproductive function brings laughter; the more-trained, self-controlled, sophisticated individual is indirect.

He proceeds by innuendo, ambiguities, covert references. The submerged intent has farther to travel, more inhibitions to vanquish, in order to reach the open field of consciousness. But all classes of society laugh at suddenly discovered lovers, at amatory irregularities, directly and without thought. When they take thought they condemn them; and often, even in condemning, laugh.

Something like denudation or exposure is involved in the laughable character of novelties. The comedy of newness is almost universal. Even if the newness is circular and seasonal, it is still funny,—so the 'first straw hat' is every season an object of derision; a boy's first 'long trousers,' or first dress-coat. Savages are said to laugh continually at their first white visitor and his appurtenances; children and even adults will tease and persecute people with an unaccustomed beard, a different cut of clothes, another accent. The new is new just because it is distinctive, different, a variation from the habitual and customary. It is a little thing, isolate, against a massive tradition, a universal manner, a cumulative habit. It is a deviation from the type, a deformity like the traditionally laughable hunch-back, club-foot, magnified nose or hare-lip. At the moment of its appearance, it is at an evident disadvantage. It is an intruder, without the power to make its intrusion good. It is laughed at. To it may be assimilated the whole assemblage of little drolls which people and diversify the daily life—irruptions of irregularity, violations of the pervasive conventions which constitute the economy of social intercourse,—such are wearing the wrong clothes, using the wrong utensil, petty misfortunes, clumsiness of manner or of speech,—the whole host of disharmonies and incongruities at which we laugh. Of these the essence is the irruption of an unexpected, a new and discordant yet impotent factor into a harmonious and well-balanced situation.

The occasions of laughter, then, as they naturally arise in the events of the daily life are occasions which contain at least two elements, not in harmony with each other. In tickling we have given the dual nature of a stimulus; in terror the sudden fall or breaking-off and lapse of a dominating tension; in pure health, the weakness of other things; in play and teasing and battle and victory, the contrast between make-believe and actuality, apparent strength and real weakness; in sex and novelty, the conflict of the natural flux and the social order. In each case the occasion offers us a contrast or conflict between two elements in which the spectator does not participate. In the course of life they appear impure, adulterate with extraneous elements, not altogether detached from the residual flux. Their arrestive and vitalizing power

is restrained by other and ulterior conditions, by almost equally potent simultaneous impetus from interests looking in other directions, toward other ends. The art of comedy consists in abstracting these essentially comic complexes from their habitations in the flux, in freeing them of extraneities, and throwing them into relief. The comic of art, hence, has a rather different character from the comic of life,—it accumulates a certain desiderative value which is akin to beauty. In art, the comic might, indeed, be called the beauty of disintegration.

Although comedy has chiefly been associated with letters and the stage, there is no telling with what degree of adequacy it might not be expressed in the other arts. A limit is suggested in the fact that movement, action, invariably intensifies comic effect, but the least degree of movement required is perhaps impossible of determination. It is certain, however, that painted and carved objects are more laughable either when they are very simple, or when they occur in a progressive series. They appear either to tell stories, which need to be supplemented by verbal rubrics, or to present very obvious direct contrasts, exaggerations, novelties, whimsicalities, oddities. They involve an essential paradox which is, at one of its extremes, caricature, at another, grotesque symbolism. Animals with human expressions on their features; human beings with bestial characteristics; inanimate objects with some of the attributes of life; living beings with the appurtenances of the non-living; inverted natural proportions; and so on to no end,—these constitute the material of the plastic comic. Sculpture is one of the arts perhaps least amenable to the comic ideal. Most laughable sculpture is caricature, often caricature by accident, not by intention. The material of sculpture, in spite of modern practice and ambition, does not readily lend itself to the representation of that disintegrating essence which is the comic material. It is more adequate to the representation of repose than of action, and the movements it most successfully represents are the integrative and co-operative movements that enhance poise and stability, not those that express inner diversity and disintegration. Grotesque sculpture is not, by nature, comic; for the genuinely grotesque is the harmony of the extraordinary. Comic sculpture, when intentional, is caricature; when unintentional is maladroitness of the sculptor. That it has a larger capacity for comic expression than it has thus far exhibited must nevertheless be admitted. But such larger expression would need to be serial and cumulative, not instantaneous. It would require explanatory legend, and would approximate very closely to the comic of painting. Painting which shall

be intrinsically comic by virtue of its coloring or design is not ordinarily conceived. There is no inherent exclusion of such laughableness; the famous Schopenhauerian example of the comic,—the curve and its tangent,—indicate that in one instance, at any rate, pure geometrical form was apprehended as laughable. There is no reason why minds habituated to the apprehension of forms and colors as such should not discover an infinite deal of the laughable in them. There might be a pure comedy of design and of landscape, as well as of human feature and action. Hogarth, indeed, approximates some such thing in his ludicrous example of the consequences that follow on ignorance of the laws of perspective. But taken as a whole, comic pictures are mainly caricatures; they have a social subject-matter, and are most effective in series. Our 'humorous' literature is full of illustrations of this principle; the daily newspapers teem with them; they are the essence of the "comic supplement." They appear, significantly, to be studies of manners. The rich comedy of such series as Hogarth's 'Hudibras,' 'The Rake's Progress,' 'The Good and the Idle Apprentice' seems to lie in the cumulative integration of cross-intentions with caricature; and it is doubtful whether this integration would be so funny without the attached verbal legends, and the presence of laughing or smiling human faces. The latter constitute a very important element in the comic effect of pictures; and their presence is usually a drawback to the determination of intrinsic comic quality.

The enhancing effect of movement on comic quality indicates clearly why comedy is more frequently a matter for literature and the drama than for the plastic arts. Literature and the drama are intrinsically serial and climactic; while painting and sculpture are simultaneous and sudden. Music, the other temporal art, whose very essence is time, is not so often said to contain or to offer comic content. Nevertheless music has its distinctly comic material and method, and its characteristic comedy. This seems mainly to be provided by a combination of light, staccato instrumentation with deep-pitched notes, by the use of uncompleted phrases, and latterly by imitative natural noises like the crowing of cocks, the cries of children, the whistling of birds,—all in careful 'harmony' with the theme of the composition. That the first two devices are musically amusing may be granted. But whether the comedy of the last device springs from the nature of the art itself or from the more apparent intrusion of a foreign element into the musical complex is an open question, though barely so. But whatever the basis of the laughter, the laughter is indubitable.

In drama and literature, the nature of the mirth-provoking object is less open to question. The material of laughter is here purely human, purely relevant to complex or simple human interests. Indeed, according to one writer the human is the only material that laughter can have. This material may be internal or external; it may offer itself in the individual solely, or in the confrontation of individuals with each other or with their environments. The outer marks of the comic individual may be merely clumsiness or deformity; may be speech incompatible with gesture, gesture with speech, the merest physiological malapropism, the lisp, the stutter, the bare misuse of language. Any one of these may be amusing; all of them taken together constitute the representative comic figure, Mr. Punch. Falstaff is funny by his mere avoirdupois, Bardolph by his flaming nose, Pistol by his rhodomontade. Bring them into action, and these purely external traits may distort purpose, and throw the most excellent intention out of gear. A fat man makes a shadowy trooper; a ranting rascal cannot tell a straight story.

But this derailing of a swift-moving intention need not depend merely upon the external characteristics of the comic protagonist. Loosely interpreted, it is the essence of every comic situation, which is in Aristotle's excellent simile "in the nature of the missing of a mark." The situation is created by the fact that the characters do not hit it off. Its clearest type is perhaps Mr. Pickwick chasing his wind-blown hat. The situation has come upon him suddenly, out of the blue. The orderly march of his life has been broken up. His hat, which properly belongs on his head and should protect him from the wind and weather, has betrayed him to the wind and weather; and to add insult to injury, leads him a sorry dance away from his proper affairs, for the purpose of restoring the disturbed balance without which they do not easily go on. The hat must be back on the man's head before the man can return to his business. This is very laughable; but normally the laughter is killed if the man is compelled to return hatless to the routine of his life. Where hatlessness begins, tragedy begins; and this is a very significant feature in all comedy. The hat may not be utterly lost if the laughter is to be saved.

The hat-hunt runs over us from practically every cranny of the comic scene. Its principle is an inversion of the ordinary, —an inversion shocking, fresh and unexpected. Instead of a trick or perversity of things, it may be an encounter of limps or persons. The runner who trips over his own feet is funny; but the clown whose running is brought to a sudden stop by the identically similar running of an identically

similar clown is funnier. The classic comedy, so well represented by the 'Comedy of Errors,' is based fundamentally upon this sort of inversion,—the kind of inversion that a person undergoes in a mirror. He is there, he is himself; yet he is not there, he is another, opposed and inimical. The alter-ego is the source of the deeds for which the ego suffers or is rewarded. The Syracusan and Ephesian Dromios are so related in practical life that the mere mirrored image of the one, having a different history, different antecedents, and a different status pays for the defects of the other. It is as if the image in the mirror were beaten for the impudence of the grimace it reflected. It is the "sudden glory" of the insignificant, the irruption and domination of the irrational.

Still another variant of it is the direct inversion of catastrophe, as the sudden and unprophesiable ups-and-downs of Face and his crew in the 'Alchemist,' the reversals of Epicoene, the inversions of the 'School for Scandal.' This is so obvious that more than to mention it is superfluous. The persistent repetition of such an inversion, always reconstituting the same situation, is another typical mode of the comic process. The battle between Punch and the devil is its key-form. Punch strikes the devil down with a blow that should deal him his eternal quietus; and the obstinate devil rises unharmed again and again and yet again to return to the attack as horrible as ever. Or perhaps the condition of the protagonist is that of the jumping-jack. Its limbs appear to move so spontaneously, so freely, so irresponsibly, while in reality they obey the inexorable leverage of strings and pulleys. I cannot think of a better instance of this type of inversion than Malvolio, so apparently pursuing his own freely-chosen purpose, so clearly the dupe and the toy of Maria and her fellow-conspirators. The comedies of Ben Jonson are full of such types, from the La Fooles, the Dappers the Druggers, to the Voltores and Moscas and Volpones.

Seek where you will in the comic of the stage or of letters, and invariably you will find something corresponding to one of these forms of inversion. If it is the comedy of mere incident, it will consist of the irruption of the unusual, an upset or reversal, of some sort, in nature essentially a disharmony like that of the man chasing his hat. In the comedy of manners, one finds private habit opposed to public usage, the mode to good sense, the individual preference to the social sanction: the comedy consists of the titillation, the see-sawing of the one with the other. In the comedy of character one finds no less the same thing, with another emphasis. The individual idiosyncrasies which are the deep-sunk well-springs of motive, pressing up action after action, with inexorable con-

sistency, are exhibited in conflict with social norms and conventional preferences. Here we are face to face with the comic object whose ludicrousness is internal first of all. It is the source of all else that is laughable, infecting with its distortions all that it touches. The comic of character is the internal homologue of the comic of person. It is founded on the internal disharmony of traits, on malproportion, moral deformity, as the other is based on physical deformity. The theory of humors, on which Ben Jonson has based all his comic pieces, fantastic and untrue though it be, has nevertheless grasped the secret of ludicrous character. It offers as the standard excellence the nature in which each of the four humors is present in right measure, just sufficiently choleric, phlegmatic, sanguine and melancholic to be of nice balance, poised for any flight you will. But change the proportion of any one of these humors, and you upset this excellent balance, and destroy the fine poise. The greater humor is at war with the others, perverts them to its own uses, interferes in their business, and ultimately breaks up the nature it distorts. The inner disharmony is expressed outwardly in a thousand ways, and this outer expression is comedy of character. Now multiply these humors a thousand fold, consider the relation of any one of the numberless preferences, habits, desires, intellections, tricks of speech, manner and gesture as well as of soul, to the remainder, and you cannot help seeing that this relation is identical with the relation between the weightier humor and the others. It is a combat, a distortion, a disintegrative maladjustment. The consuming passion for silence in Morose, the self-conceit of Malvolio, the didacticising stupidity of Polonius, the avarice of Harpagon, the magniloquent aimlessness of Mr. Micawber, the hypocrisy of Tartuffe, the subtly rigid self-worship of Willoughby Patterne, and I care not what other trait of what other person you will,—each is a trait which is comic only because disproportionate, and hence, wherever it appears, disorganizing. Harpagon loses his wealth because he loves it so; and, by the way, is made altogether ridiculous because his moral deformity intrudes and operates where it should not. Had Shylock loved revenge less, he would have suffered less; and Malvolio, certainly an efficient steward, had nothing but his cancerous self-love to thank for his degradation and misfortune. Hypertrophy of imagination over common sense in the Knight of La Mancha, the atrophy of imagination in Sancho, the fleshly weakness in Falstaff,—such are the fountains of comedy in these heroes of the sock. Whenever any one quality is called into play, this forestalls it, snatches its action from it, or spoils it by its influence. Perhaps all comic traits are no more

than the love of life, the instinct for self-preservation, no more than the spontaneous and natural egoism of mankind, taking a perverted direction, so eager to live well as to belie fantastically the most fundamental conditions as well as the most subtle of right living. The greatest of all ruinous misproportions is, of course, that of self-deception. Invariably by its means diverse social and natural antagonisms are exhibited and made explicit, whether in the adventure on Gadshill, the wind-mill tilt, the tantalizing dinner, or the cross-gartering. What "moves men merrily" is the far-spreading infectious disharmony.

This patent malproportion in character which is the prime source of comedy has led to an opinion, variously held, that the comic figure is an abstraction, that he is less individual and more 'universal' than the protagonist of tragedy; and that the function of comedy is that of social correction. There are some grounds for this inference. The practice of the Greeks in the use of types and type-names,—names like Phidippides, Dicæopolis, Mania of the Aristophanic comedy, or Glycerium, Palaestra, Bombomachides of the Plautan comedy, the Voltores and Corbaccios, the La Fooles and Moroses, the Mammons, Subtles, Faces of Jonson, the similar practice of his successors far into the eighteenth century, attest that dramatists seemed to be dramatizing moral qualities and types rather than persons. The very titles of the comedies: "Wasps," "Birds," "Volpone," "Epicoene," "L'-Avare," "Les Precieuses" bespeak traits rather than persons. But moral tragedies like "Everyman" and "Ghosts" are no less typically and abstractly named; and there is scarcely a tragic character that cannot, as properly as any protagonist of comedy, be labelled by the peculiar trait which constitutes his tragic nature. In point of fact, comedy has no monopoly over these forms of art in the chastisement of the anti-social. And what, moreover, is anti-social? A convention, a mode or habit which has attained universality is as often the object of laughter as an isolated individual, a group as often as a habit. And these are as frequently condemned by tragedy as by comedy. Satire and irony, indeed, are correctives. But the corrective principle of these is not their comic quality, but their tragic earnestness. Satire is a battle, not a joke; comedy turns the battle into a joke. Where comedy becomes corrective it is no longer truly comic. For the subject of a joke there can be no sting if he is to laugh; and if it stings he cannot laugh. The laugher can have no portion in the ruin which moves him to mirth.

That it is a ruin which moves to mirth, and that the merry man must have no share in it, is most patent in the comic of

words. Civilized comedy is at its highest in words. These alone can render the very refinements of mal-adjustment, the delicate disharmonies of the spirit. They reveal the range of battle between mind and mind as nothing else can. Yet what target of a poisoned verbal dart ever responded to the impact with laughter or admired the accuracy of the aim or the sharpness of the missile? Invariably his first action is the aggression or withdrawal of defence. A return shot, scornful silence,—but no broadside of laughter. The play of wit has always imminent over it the play of the sword. The quip becomes the stab with a turn of the hand, and this just because the object of witty play is a ruin, or like to be one in that play. Recall by way of example that superb witticism of Heine's at a certain Parisian *salon*, where he, Soulié and an enormously wealthy *parvenu* were guests. The *parvenu* naturally received more attention than the two men of letters,—which moved Soulié to remark: "Even in the nineteenth century, they still worship the golden calf." "Yes," assented Heine, "but this one is much older." This characteristic Heinesque remark makes of its subject an ox; and 'ox' is the German *Schimpfwort* for stupidity, dullness, maladroit idiocy. To call a man an ox is to insult and to degrade him; it is, by a stretch of meaning, to ruin his reputation for intelligence, to destroy his human dignity, and to make him like the beasts of the field. This Heine has done. Moreover, he has not done it by a direct aggression. He has ostensibly referred only to the age of the *parvenu;* he has ostensibly even defended him against attack. He has said nothing overtly insulting, yet he has called the man a calf of advanced years. In the phrase, "much older," therefore, there are two ideas not compatible, not belonging together, titillating the attention of the auditor. And this enhances the excellence of the witticism. Of wit which is impersonal, which is the play of ideas as such, and has no moral lilt against another person, the essence is this unstable union of thoughts, this conflict, incongruity, crush, and interference of two or more ideas, struggling for place in one word. The pun is, of course, the most obvious example of this fact; but it may be brought about in many ways,—by a slight difference in emphasis, metonymy, inversion, metaphor. Invariably there is an ambiguity between denotation and connotation, between figurative and literal meaning, which is the soul of the *double entendre*, as well as of the bald disharmony of ideas or of objects. Much of its quality is evident in the reply of one soldier to another who had called his attention to the bold escape and the immediate pursuit of a spy. "He is running for dear life" said the one, and the other replied "He 'll

never buy it at that rate." It will be seen here that the literal and the figurative intention are jammed together in a strange and not incongruous contact. The pleasure and the taste of it are due to the jam. Still more potent does this become where there is no relevance whatever between objects, as in the wide perversion of the Twainesque humor, or in the attempt of the Scotchman to make his friend understand the meaning of 'miracle.' Tam had tried hard to teach him, but with ill-success. Finally he resorted to parable. "Look ye," he said, "when ye see a coo sittin' doon, that's no' a mirrecl. When ye see a thistle standin' up, that's no' a mirrecl neither. An' it's no' mirrecl when ye hear the throstle whistlin' in the tree. But when ye see the coo sittin' on the thistle, and singin' like the throstle, that 's a mirrecl mon, that 's a mirrecl." The incongruities are here obvious. Their violent refusal to hang harmoniously together is the strength whereby they "move men merrily." A most subtle form of it is the famous " 'T was brillig . . . " in 'Through the Looking Glass.'

From the coarse and obvious comedy of the clown with his falls and tumbles, to subtle and recondite plays of wit the material of the laughable remains invariably a disharmony, a maladjustment ranging from the impact of bodies to the clash of souls. No less do the depth and scope of philosophy, where surely there should be little place for laughter, offer the great and eternal disharmony, a spectacle which, as poets have more than once sung, moves the gods merrily. But men are so moved no less than gods. The cosmic vision may stir the thinker to cosmic laughter. History offers us one strange and wonderful figure, isolate among his kind, whom tradition names "the laughing philosopher." Democritus of Abdera saw the great contrast between man's hopes and his condition, his conceit of himself, his belief in his own power, his headlong passion and pursuit of his petty ends as though they were the world's will and the world's purposes, as though his struggle were the cosmic joy and sorrow. But the cosmos is a void, and a hurly-burly of atoms. Against the volume of their inexorable tumult, man's cries are as utter silence; against the background of their fatal onward rush his willings and achievings, but the uncouth jerkings of the jumping-jack's limbs when the strings are pulled. Man is the ruined victim of his own illusions. His destiny is death because it is self-deception. Therefore Democritus laughed. Laughter, cheerfulness, *εὐθυμία*, is a restoration of the true proportions. It rests upon a recognition of the narrow limits and the eternal conditions of human well-being. It is a turning of destiny to scorn by accepting it, as one destroys the sting of rebuke and the violence of anger by offering them no

resistance. They are turned to derision because they are spent on a void, losing meaning and purpose. Thus the laughter of the sage is a double laughter. Its subject is the self-deception of man which combats the inexorable cosmos, but its subject is also the rage of the cosmos spent upon nothing at all. In both cases there is an inversion, a disintegrating disharmony, outside of which the sage stands, and the master of which he feels himself to be. He is upon the Lucretian rock, watching and enjoying the storm and the shipwreck below.

The range of the comic scene, we gather, is no less than the cosmos itself. The occasion of the laughter may be the compass of one small baby's toe, or the unbounded universe. It plays over the whole gamut of human relationship and cosmic disharmony. Nothing may escape it, from the attenuated malproportions of abstract mathematics to the terribly weighted deflections of the universe. But of laughter two things seem true. The first is the fact that it cannot endure. Custom kills comedy. What is habitual, what we are well-adapted to, what is for a long time a part of our own lives cannot move us merrily. To do so, it must exclude us, make us foreign to it. It must become something in which we no longer have a portion, and which for the time, has no portion in us. The traveller is likely to feel this most keenly; that is, if he is a laugher, rather than a creator of laughter. The creator of laughter, the professional humorist, can scarcely be a laugher. He is not a humorist because he sees the comedy in things, but because he twists things and distorts them so as to make them comical. He is invariably a preternaturally solemn person. Laughter must be free, but the cause of laughter is always bond. The maker of the laughable is the servant of his vocation; he cannot laugh and render service at the same time. The laugher is served, but serves not. Hence, then, the traveller who can laugh finds all things in a new country ludicrous at the beginning. Customs and modes, habits of life and manners, the very scenery move him to laughter. But as his stay is prolonged, the disharmonies seem to rub off; the articulation of life becomes smoother and less noisy. He himself has now become, to some degree, a part of the structure; speech, manners, dress,—his own have somehow become confluent with them, have set him at their centre, where he once was at the periphery. He can no longer laugh; nor can he understand his original laughter. This process is true no less of an oft-repeated game, a witticism, a relieved nervous tension or a philosophy. Familiarity breeds seriousness or indifference before it breeds contempt. The second characteristic of laughter is that it

enhances or preserves the laugher's implicit values, not always obviously or directly, but invariably. The outcome of the comic situation is an alterative outcome, not a destructive outcome. The disintegration which is the object of laughter leads to re-distribution, re-adjustment, harmony, not to real human loss. The upshot of any comedy shows a harmony attained by attrition and elimination of excrescences, by the reduction of the evil, by a restoration, even if only a momentary one, of things to their normal,—one may even say, to their normative,—relationships. The inversion of the natural order in which most comedy begins, proceeds in the course of the action, by the mere inertia of the comic disharmony, to right itself. Don Quixote is led by the effects of his madness to realize and see it truly. Harpagon is led by the operation of his avarice to comprehend its evil nature; Willoughby Patterne loses some of his self-love, Volpone passes from his dishonorable bandages to his more dishonorable chains. The new harmony may not be enduring, but it ends the comedy. And it is, of course, true that not always are the normal social standards re-asserted and the habitual conceptions of virtue victorious. In Epicoene the punishment of Morose is to our modern sense perhaps harder than his deformity of spirit deserves; the enrichment of Sir Dauphine by a swindler's trick, our contemporary moral sense will hardly stomach. But, notoriously, nothing is so variable as the actual social standard of mankind from period to period. Whenever we look more closely at the post-comedial harmony, we find that the standards of the age to which the comedy belongs have been vindicated. The standards of all time have little to do with comedy. It is sufficient that any prized thing shall be preserved or enhanced, that any distortion or evil shall be destroyed or decreased, even if for the moment only, not alone in the drama but wherever the comic occurs in sculpture, in painting, in the events and routine of daily life. The hat-chaser must recover his hat if he is to remain merely a comic figure.

Considering all of these facts together, what do they yield as the æsthetic principle in comedy? What is there identical between the tickled toe of a suckling infant and the philosophy of a Democritus? Students of the comic have given this question widely varying answers. There has been perhaps as much confusion in the definition of the comic, as in the definition of the beautiful. Theories may be roughly divided into three classes, yielding a certain minimum of unanimity.

The first group of theories may be called "degradation theories." They conceive the object of laughter as reduced in worth; and the laugher as enhanced therein. As Hobbes

has it: "Laughter is nothing else but sudden glory arising from some sudden conception of some eminency in ourselves by comparison with the infirmities our own formerly. . . . (It) proceedeth from a sudden conception of some ability in himself that laugheth. Men laugh at the infirmities of others, by comparison of which their own abilities are set off and illustrated." Laughter here is self-enhancement at the cost of one's fellow. The self-enhancement is as important as the degradation of the other. Other writers, however, take only the degradation to be significant. So Bain finds the "occasion of the ludicrous" to be "the degradation of some person or interest possessing dignity, in circumstances that arouse no other strong emotion." The dignities, moreover, must not "command serious homage;" and Groos finds the comic object to be one in a topsy-turvy condition, and hence regarded with a feeling of superiority. But for all three the object of laughter has in some way been reduced from its high estate. Something of the same sort may have been in Spencer's mind when he wrote that laughter naturally comes when there is "a descending incongruity," a turning from great things to small, a degradation.

The theory of degradation fails, however, to square with the obvious fact that degradation is a matter of geography, inclination, breeding and incidental affection. As one man's meat is another man's poison, so what may seem degradation to one may be exaltation to another. The mental state of the laugher is hardly one which feels the sentiment implied in degradation. It does not seem, in most cases, to possess what the Germans call *Tendenz* or *Schadenfreude*. As comic sense it carries detachment and freedom rather than malicious intention. The correct envisagement of fact which the theory offers is more simply because more freely offered in those explanations of the comic whose key-word is "contrast." The "contrast" theories emphasize differently the elements contrasted, but their intent is the same throughout. One author finds the contrast to consist in the complete exposure of weakness through the presence of a superior power. Schopenhauer sees it as the "unexpected subsumption of an object under a conception which in other respects is different from it." Hence he infers that "the phenomenon of laughter always means the sudden apprehension of an incongruity between such a conception, and the real object thought under it, thus between the abstract and the concrete object of perception." Bergson finds it in the opposition of the suppleness of life with the stiffness of mechanism, the substitution of one for the other; Freud in the release of repressed and submerged —chiefly sexual—complexes. And there are many other ways

of specifying contrasts. But they are, it will be seen, no more than specifications; their common element is the "contrast."

The contrast theory of the comic defines the comic by considering its objective nature. Aristotle's description of it as "in the nature of a missing of the target" stands between this objective description and the more directly psychological theory of Kant and his followers. This theory might be called the theory of "disappointed expectation." "Laughter," writes Kant, "is an affection arising from a sudden transformation of a strained expectation into nothing. . . . A jest must be capable of deceiving for a moment. Hence when the illusion is dissipated, the mind turns back to try it once again, and thus through a rapidly alternating tension and relaxation, it is jerked back and put into a state of oscillation." The first of these Kantian suggestions is hardly more than paraphrased by Lipps for whom "the comic arises, if in place of something expected to be important and striking, something else comes up (of course under the assumption of the ideas we were expecting) which is of lesser significance." The other half of the Kantian description has been more popular. We might call it the "oscillation theory" although it is essentially a form of contrast. It has received the endorsement of Hecker and of Wundt, and has been attached by them to the term "contrast."

The variations in these fundamental notions are innumerable. Writers have found the comic to be only that which violates social usage, or only that which conflicts with established moral, intellectual or æsthetic standards. The net result of a review of all of these theories is that they are all true, and in so far as they deal with unrelated facts, all exclusive of one-another. They are *specifications* of comedy under special conditions and in various fields. They contain the essence of the comic; but they have not really isolated it. Our journey through the field of laughter has shown us that this essence may reside anywhere in the universe. It is not confined to human beings or to social norms, as certain authors believe; nor is it limited to the merely living. Its habitat is as wide as experience. It ranges from the tangent which so stirred the jocund Schopenhauer, to the universe which amused Democritus. As anything may be beautiful, so anything may be comic. It becomes comic, as all the comic objects which we have examined have shown us, and as the theories of the comic which we have considered obviously affirm, when somehow it is at a disadvantage, out of proportion, mal-adjusted. It becomes comic when it constitutes a disharmony. This disharmony is the basis of contrast, the cause of oscillation, of

disappointed expectation, the essence of degradation. But by the mere fact of being a disharmony the object is not yet comic. The daily life and the arts offer the mind an infinity of disharmonies which are either tragic or indifferent. Intrinsically, things are no more comic than they are beautiful. The comic, like the beautiful, is not a property which things possess, but a relation which they bear to the mind. We do not laugh at a thing because it is funny; it is funny because we laugh at it.

An examination of the nature of laughter itself will show us that which more specifically constitutes comedy. We have found laughter to be a wide-ranging action, corresponding to the active character of its object. But this action does not have the purposeful, rapt nature of other human activities. It seems to be a detached and free thing,—a thing which is leisurely and secure. Even when it ensues upon absorbing fear, upon the madness of anger, the anguish of passion, it seems to have this liberty and security, this leisure, as opposed to the precedent breathlessness and extreme intentness. It seems indeed often to be a cry of freedom, of relief, a roulade of triumph. When we seek the earliest semblance of an apprehension of the comic, we find it in the replete child, repeating the pleasurable act of sucking. Its normal expression in the smile requires the baring of the rending and cutting teeth, the assumption of an appearance which, when well-considered, bears a startling resemblance to an animal about to rend and devour its prey. In the hungry beast of the jungle, that has fought for its life in a double sense, and has triumphed in its struggle, may lie the ultimate parentage of laughter. The explosions of breath, the gurgitations, the throwing back of the head as if to swallow, the sprawling, expansive movements of the limbs,—those are actions that beasts still perform when they have their prey completely at their mercy. And this prey, up to the moment of possession, was a peer. The struggle to live matches not kind with kind, but every kind with all other kinds; its may be a contest of strength against swiftness, ear against eye, eye against nose. And the struggle invariably carries its essential hazard which makes even the weakling his enemy's peer. There is therefore the inevitable absorption and tension and breathlessness. In no matter how unequal a combat, there is even for the victor one moment of dread and menace, and there is the final triumph and relief in laughter. The primeval laugher is the triumphant beast, with its paw upon its defeated enemy, and its jaws set for the act of devouring. The first laughter is life's earliest cry of victory over the elemental world-wide enemy that wages the titanic battle with it. Laughter is perhaps a mutation

from feeding, and it serves the same result: it strengthens life by heightening its vitality. Its scope has expanded as the world has expanded. The laughter of man has all things for its object,—all things that may enthrall him or do him hurt, in whatever sense. It 'degrades' them, makes them man's proper food; it contrasts them with what they were; it destroys their power over him. He stands outside and beyond them; they cannot touch him. The object of laughter is ridiculous, not in so far as it is good, but in so far as it is dangerous. It is the frustrated menace in things, personal, social or cosmic,—that moves men merrily, when their power for evil is turned to emptiness. The novel, the dark, the cancerous in the life of the spirit and in the life of the body becomes ridiculous when we recognize that it is ineffectual. And conversely, to turn a thing to ridicule is to make it ineffectual, to throw it out of gear, to rob it of its place, to compel it to spend its energy in a vacuum. This is true degradation, and the laughter in it is not appreciation but malice. It is for this reason that even to so intelligent and sympathetic a student of the comedy as Bergson or Meredith, comedy seems to be a social corrective. But they fail to see that the comic force lies not in the correction, but in the joy of the corrector. There is always the possibility of a certain cruelty in comedy, an utter brutish joy in victory which is ethically more outrageous than the thing it destroys, until one remembers that what laughter consumes, laughter first finds evil.

This observation yields the key to the right definition of comedy. Beauty, it has been noted, is the relation between the mind and the environment when the two are adapted to each other harmoniously, perfectly and immediately. And the environment which beauty presents to the mind is good in itself, an intrinsic and direct excellence. Now the environment which comedy presents to the mind is primarily an evil, full of discord and unrest. This evil comes to us, however, not as our peer, but as our slave, bankrupt and stripped of its power to harm. And to it, as to the thing of beauty, we are adapted directly and instantly. Comedy, then, like beauty, is a relation, but it is a relation in which we are harmoniously and completely adapted to what is in itself a disharmony, a mal-adjustment. It is a relation which converts evil into goodness. It adapts us adequately to disharmony and mal-adjustment, snatching as it were, life's victory from the jaws of death itself.

CONSCIOUSNESS IN RELATION TO LEARNING

By Louise Ellison Ordahl

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I. Introduction

The question of the relation of consciousness to learning, in the case of man, was suggested by the disagreement among psychologists as to the value of "ability to learn" as a criterion of consciousness in animals. From a purely metaphysical standpoint, those who accept the doctrine of psychophysical parallelism would admit consciousness of some sort as an attendant of the activities of all animals. Those who attack the problem from a purely empirical standpoint[1]

[1]Bethe, A.: Die anatomischen Elemente des Nervensystems, und ihre physiologische Bedeutung, *Biol. Cent.* 1898, XVIII. pp. 863 ff.
Nuel, J. P.: La psychologie comparée, est-elle légitime? *Arch. de psy.*, 1904. V. p. 320.
Ziegler, H. E.: Theoretisches zur Tierpsychologie und vergleichenden Neurophysiologie, *Biol. Cent.*, 1900, XX. p. 1.

either attempt a complete explanation of animal behavior from the physiological side without the assumption of mental qualities, or else they argue that consciousness is present only when the animal is able to profit from experience, or to profit from it rapidly enough to argue the presence of a psychic resultant of former experience.[1] Still others assume that consciousness may be present in all animal forms; but the power of associative memory is a measure of its grade, or a proof of its existence.[2] Those[3] who deny the possibility of a comparative psychology are met by the answer that the ascription of consciousness even to human beings rests upon inference and assumption, no mental states being capable of proof but our own.[4] Before considering the question of the possibility of learning without consciousness, and the relation of learning to consciousness,—which are the main themes of this study,—we shall consider briefly a few definitions of the term "consciousness," and the related terms "unconsciousness," and "subconsciousness."

II. The Concepts of Consciousness, Unconsciousness and Subconsciousness

Practically all admit that "consciousness," being an ultimate, is incapable of definition; yet it has been variously described and explained.[5] For Descartes it was equivalent to self-consciousness; Wolff was the first to give it the meaning of "ultimate property of the soul;" while others consider self-consciousness to be only a particular form of consciousness. For Lipps it is identical with the ego.[6] Usually it is broadly an equivalent for awareness or experience, and an opposite to the unconsciousness of coma, fainting, dreamless sleep, etc. Some writers make it synonymous with attention, or a general term for that experience of which attention

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[1]Loeb, J.: Comparative Physiology of the Brain and Comparative Psychology, New York, 1903. p. 12; p. 118.
Washburn, M. F.: The Animal Mind. New York, 1908. p. 33.
Romanes, G. J.: Animal Intelligence, New York, 1883. p. 4.

[2]Wundt, W.: Grundzüge der physiologischen Psychologie. (Fifth Edition.) Leipzig, 1902. III. pp. 324 ff.
Romanes, G. J.: *Op. cit.*, p. 4.

[3]Ziegler: *Op. cit.*, p. 2.

Nuel: *Op. cit.*, p. 343.

[4]Yerkes, R. M.: Objective Nomenclature, Comparative Psychology and Animal Behavior. *Jour. Comp. Neurol. and Psy.* 1906. XVI. p. 383.
Forel, A.: Ants and some other Insects. (Trans.) Chicago, 1904. p. 2.

[5]See Horwicz, A.: Psychologische Analysen, Halle, 1872-1875, for review.

[6]Lipps, T.: Leitfaden der Psychologie, Leipzig, 1909. p. 6.

is only a high degree, or with selective consciousness. Defined as "meaning" it is almost equivalent with apperception. Memory and consciousness are sometimes identified, but on the other hand, memory and unconsciousness are also identified, memory being regarded as a function of all matter, and perfect memory a characteristic of automatized acts unconsciously performed. Consciousness is frequently an equal term with "experience" and "psychic." It describes the "sum-total of our mental experience."[1] It is the interconnection of our psychic processes,[2] or a series of ideas connected with each other.[3] It is "co-ordinated psychic activity,"[4] synthesis, change, or an "orderly succession of changes." It is characterized by the pursuance of future ends, and is a synthetic unity.[5]

Consciousness is supposed to accompany only afferent impulses sent in from a moving organ,[6] or "neural processes of peculiar organization," or complex constellations of neurones.[7] It is supposed to arise "only when the motor cells are ready to discharge toward the periphery,"[8] when the sensory impression is being followed by the motor reaction;[9] "it involves not only the sensory side, but the motor discharge."[10] "It varies with the novelty of the neural processes concerned, and accompanies new connections;"[11] it "attends new complex functions."[12]

Since any definition of consciousness touches either directly or indirectly upon the disputed question of "uncon-

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[1]Wundt: Grundzüge, III, p. 321.

[2]Wundt: *Ibid.*
Titchener, E. B.: An Outline of Psychology. New York, 1908. p. 13.

[3]Calkins, M. W.: An Introduction to Psychology, New York, 1901, p. 150.

[4]Marshall, H. R.: Instinct and Reason, New York, 1898, p. 43.

[5]James, W.: Principles of Psychology, New York, 1890, I, p. 8; p. 139.
Morgan, C. L.: Introduction to Comparative Psychology, New York, 1906. (Second Edition.) p. 154.

[6]Morgan, C. L.: Animal Behaviour, London, 1900. p. 45.

[7]Sidis, B. and Goodhart, S. P.: Multiple Personality, New York, 1905. pp. 3 ff.

[8]Kirkpatrick, E. A.: The Part Played by Consciousness in Mental Operations. *Jour. of Phil., Psy. & Sci. Methods*, 1908. V. pp. 421-429.
Breese, B. B.: On Inhibition. *Psy. Rev. Mono. Supp.*, 1899, Vol. 3, No. 1, pp. 1-65.

[9]Maudsley, H.: The Physical Basis of Consciousness, *Jour. of Mental Science*, L, 1909. p. 12.

[10]Münsterberg, H.: Grundzüge der Psychologie, Leipzig, 1900. I. 531. ff.

[11]McDougall, W.: A Contribution towards an Improvement in Psychological Method, *Mind*, 1898. N. S., VII, p. 366.

[12]Royce, J.: Outlines of Psychology, New York, 1903. pp. 81-2.

scious'' or ''subconscious'' mental processes, a brief discussion of the two terms is expedient.

UNCONSCIOUSNESS

''Unconsciousness'' is a negative term denoting the opposite of consciousness. It is employed in describing states like fainting, epilepsy, or dreamless sleep where every mental quality is wanting. It is also applied to psychophysical processes which lack their normal conscious accompaniment, as pain or deep emotion temporarily forgotten during great excitement, and to perceptual processes such as the unconscious inference of depth from the fusion of the two retinal images, and the perception of a clang of definite quality from the fusion of partial tones which analysis alone discloses. Automatic acts are ''unconscious.'' The word is also loosely used for ''unreflective,'' ''unintentional,'' or ''inattentive.'' ''Unconscious mental process'' may have various meanings. It may indicate the physiological process correlated with a conscious process, or a physiological process with no psychic accompaniment, but determining consciousness later, or a neural process with psychic accompaniment of which for some reason the individual possesses no awareness. These uses will be discussed later. Whatever the metaphysical implications, the term when carefully used is a limiting concept opposed in its significance to ''consciousness.''

SUBCONSCIOUSNESS

''Subconscious'' is more ambiguous in meaning than either of the two preceding terms, and though it is frequently interchangeable with ''unconscious,'' it usually implies something more definitely psychic. It denotes (1) the forgotten, (2) the purposeless, (3) the unnoticed, (4) the mechanized, (5) the reproducible, (6) the productive, (7) the psychic real.[1] It is also used to describe (8) simultaneously existing secondary streams of consciousness thought to appear in the pathological phenomena of divided personality, (9) and dissociated states which some writers believe to be synthesized into a subliminal, submerged self and to constitute a large part of mind.[2] The first three uses of the term subconscious (or unconscious) are descriptive of facts of experience, the 5th, 6th and 7th are metaphysical interpretations of results whose causes are unknown, and the fourth and fifth are a mixture of facts and

[1]Hellpach: Das Unbewusste, *Centralblatt f. Nervenheilkunde u. Psychiatrie*, 1908, XXXI, pp. 65-66.

[2]Summary by Prince. A Symposium on the Subconscious. *Jour. of Abnormal Psy.*, 1907-8, II, pp. 69 ff.

interpretation; the last two uses are descriptive and explantory of anomalies of consciousness and will be considered later.

Used in the sense of unnoticed or unattended, "subconscious" (or "unconscious") belongs to the realm of experience and is used in the sense of "least consciousness." As such no metaphysical implications are involved. But it is an easy step from dim consciousness to complete loss of consciousness, and many authors make the transition, including, as psychic, processes beyond the point where we are even dimly aware of them. Discussion as to the psychic nature of processes of which we are not introspectively aware began with Leibnitz[1] and the later treatment of Hamilton, Mill, Brentano and Carpenter. The use of the conception in explanation of the pathological phenomena of multiple personality, hysterical manifestations, hypnotism and other anomalies of consciousness has more recently brought the question into prominence again. The following are most of the representative arguments for the hypothesis that the mental life is wider than experience, with the opposing views:

(1) Total perceptions must be composed of an infinite number of infra-conscious sensations. The roar of the ocean is made up of the imperceptible sound of each wave, the greenness of the forest of the color of each separate leaf.[2] "But," it is answered, "this is not necessarily so, for a sum of magnitudes differs from its parts, not merely quantitively, but qualitatively." A lesser degree than zero changes water not partially, but completely to ice.[3] A sufficient quantity of the cause may be necessary to produce any of the effect.[4] The infra-sensible stimuli affect the nerve and help the birth of the sensation when other stimuli come, but it is a matter till then of the nerve-cell only.[5]

(2) By far the larger part of our spiritual possessions are not in consciousness, but are the forgotten memories, unconscious habits and the results of early experiences. That these are really concerned is shown by cases where delirious patients speak the forgotten language of early childhood,

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[1]Leibnitz: New Essays Concerning Human Understanding (trans.), N. Y. 1896. pp. 47-52.
Hamilton, Sir Wm.: Lectures on Metaphysics, Edin., 1877, Vol. 1, p. 339 ff.
Mill, J. S.: An Examination of Sir Wm. Hamilton's Philosophy, London, 1878, Vol. 1, ch. 8.
Brentano, F.: Psychologie, Leipzig, 1874, Ch. 4.
[2]Leibnitz: *op. cit.*, pp. 47-52.
Hamilton: *op. cit.* p. 339 f.
[3]Brentano: *loc. cit.*
[4]Mill: *loc. cit.*
[5]James, Wm.: Principles of Psychology, N. Y. 1902. Vol. 1, p. 159.

and idiots remember things they have heard but not understood, also the oft cited case of the ignorant servant who quoted in delirium passages of Hebrew she had heard her master read. Contrary opinion contends that not the ideas, but the power of reproducing them remains latent, physiological modifications only being concerned. Understanding what one hears is different from consciousness, and probably the idiots and the servant were aware of the sounds, but not of their import. In memories, not the concept is conserved, as Herbart believed, but "molecular habits of the brain."[1] "Unconscious psychic dispositions" would have to be assumed just as much for newly produced as reproduced ideas.

(3) Habitual actions which are at first entirely conscious become mechanized and unconscious. Complete activities formerly requiring a voluntary initiation of every act in the series, now run on of themselves when consciously started. One party holds this fact to be evidence that the psychic side exists unconsciously; the other, that the process has become merely a matter of physiology.

(4) Unconscious psychic processes are manifested by results existing in consciousness—results whose underlying processes are entirely unknown to us. Examples are the perception of depth from the fusion of simultaneous double retinal images, the perception of direction of sound by its relative intensity in the two ears, and other judgments or inferences spontaneously made without consciousness of the reasons or of the underlying principles. Also, there are sometimes present in the after-image details which were not seen in the original image.[2] Against the last example Brentano urges that the after-image is really due to a prolongation of the physiological excitation. In the original perception consciousness was occupied with something else, but is free later so that the unnoticed phase makes itself evident in the after-image. A similar argument, *i. e.*, that details of which we were unconscious in the original experience are frequently observed later in the memory image, might be met by the explanation that such details were actually present, but were not in the focus of attention.

A physiological explanation is thought to meet the next three arguments.

(5) By turning our attention to something entirely different we are frequently enabled to recall a forgotten name, etc., which has been supposed to prove that an unconscious pro-

[1]Münsterberg, H.: Grundzüge, d. Psychologie, Leipzig, 1900, pp. 223-224.

[2]Helmholtz: Handbuch d. physiologischen Optik. Hamburg, 1896. pp. 602, 962.

cess has gone on to call it up. Profound sleep may recover lost ideas, because, it is held, the process goes on undisturbed by our fruitless efforts. (A refreshed and rested brain may be responsible for quicker association.)

(6) The same thing is seen in the recall of forgotten facts, and even unnoticed details of a former experience, while a different stream of conscious thought flows along beside it. Automatic writing, "crystal gazing" or "shell hearing" may make known to us facts which it has been impossible for us to recall or even of which we were "unconscious" before. (The automatic writing is explained as a purely physiological process, with no "detached consciousness" and the "unconscious" perceptions as conscious but rapidly forgotten.)

(7) Activities similar to conscious activities except that they are carried on during abstraction and so lack the conscious quality are supposed to show unconscious psychic processes. For example, one may take one's way along the street, choosing one of many possible directions, while one is so absorbed in deep thought that he is unconscious of what he is doing. Or one may hear a sound, detect an odor or feel a pressure while absorbed without being conscious of it, but when the engrossing thought is past it may come to full consciousness. Bleuler, who thinks consciousness occurs only with the association of a complex with the ego-complex,[1] explains such cases by saying that the object was perceived by the psyche, but not so associated. The explanation by those opposed is that what we have is perception with rapid oblivescence.

(8) "Mediate associations where ideas arise which have no causal connection in consciousness show the efficiency of unconscious links." Jerusalem reports such a case, as follows: A man was busy at his work when suddenly there flashed before him a scene witnessed many years before, which was totally out of keeping with his present occupation. Search for the connecting link finally resulted in finding a tiny hidden flower, which he had not known was present, and which had been directly associated with the earlier experience. The odor, of which he was "unconscious" had probably been responsible for the association. Wundt answers to this and like cases that the odor was conscious but unnoticed,[2] and other opponents take the same attitude. Scripture performed a series of experiments which seemed to show that associations were formed between nonsense syllables by iden-

[1]Bleuler, E.: Bewusstsein und Association, *Journal f. Psy. u. Neurol.* VI, 1906, 154. Re-published in C. G. Jung's Diagnostische Associationsstudien, Leipzig, 1906. I. 257.

[2]Wundt's *Philosophische Studien*, X, 1894. S. 326-8.

tical characters written on the outer part of the cards used so that they were not noticed.[1] Cordes and Messer think they find cases of mediate association, but Münsterberg,[2] W. G. Smith and Howe[3] get only negative results. Pierce[4] suggests as an explanation for cases like finding one's self saying a word which later we discover to be written somewhere but which we have not "seen," as the 'translation' of the image into another field than that in which it was received. Out of 892 observations of "free rising" ideas Kiesow reports that 41% could be accounted for, and believes that his experiment proves all such links to be conscious and either ideational or emotional in character.[5] By some, "free rising ideas" receive a physiological explanation.

(9) Closely connected with mediate associations are the sudden flashes and insights—ideas which apparently come from nowhere—and the exaggerated and more brilliant form of the same thing seen in the inspirations of the genius and the fancies of the poet. Not an unconscious ideational connection, but a purely physiological basis of the association satisfies the opponents of the "unconscious."

(10) Decisions have sometimes been reached or problems worked out in sleep. In dreams the result may be arrived at, but the setting may be fantastic or absurd. Here, too, the physiological mechanism may been have started and carried the whole thing out by itself, or if the process is accompanied by dreaming it is none the less conscious for the fact that associations are lacking to make the setting normal.

(11) Development of emotional states is often unconscious. Prejudices are formed for no conscious reason; appreciation of art rests on unconscious factors; a man may be in love without being conscious of it. Here the usage of the term rests on the identification of consciousness and self-consciousness. Because an attitude is "unreasoned" it is not necessarily unconscious. Experiencing an emotion or idea and having it as an object of consciousness are two different

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[1]Scripture, E. W.: Über den associativen Verlauf der Vorstellungen, Leipzig, 1891. Diss. pp. 76-87.
Cordes, G.: Experimentelle Untersuchungen über Association. *Philos. Studien*, XVII. 1901. S. 73.
Messer, A.: Experimentell-psychologische Untersuchungen über das Denken, *Arch. f. d. gesam. Psy.*, VIII. 1906, 63 ff.

[2]Münsterberg, H.: *Beiträge*, Bd. 4 1892 S. 2-8.
Smith, W. G.: *Mind*, N. S. III. 1894. p. 301.

[3]*Am. Jour. Psy.*, VI, 1894. pp. 239-41.

[4]*Jour. Phil. Psy. & Sc. Meth.*, I, 1904. pp. 400-3.

[5]Kiesow, F.: Ueber sogenannte "freisteigende" Vorstellungen, usw. *Arch. f. d. Gesam. Psy.*, VI, 1906. 357-90.

matters. (James.) The man in love is conscious of each feeling and sensation, but not of the fact itself (Brentano).

Unconscious and Subconscious as Used by Psychopathologists

The use of the terms "unconscious" or "subconscious" by the psychopathologists to describe anomalies of consciousness deserves special attention because the sense is somewhat different from any of the preceding cases. Consciousness is conceived as split into two consciousnesses, one usually more firmly knit together and predominant than the other, the secondary consciousness, "co-consciousness," or "subconsciousness." These two (or sometimes more) consciousnesses may exist simultaneously or alternate with each other. Prince explains the phenomena thus. Ideas making up an experience tend to become organized into a complex which may be a subject, time or mood complex. Dissociation of personality may take its line of cleavage along any one of these three complexes and in abnormal conditions a complex which is only one side of a character may become the main or sole complex of the new personality. Complexes may be artificially organized in hypnosis, trances, etc. The formation of complexes has its basis in the organization of the neurones into complexes, which retain their organization so that stimulation of one element starts the whole process. Physiological complexes can be conserved despite absence of awareness in the original experience. Strong organization of physiological complexes together with lowered physiological threshold and decreased inhibition, might render them accessible to minimal stimulation, whether peripheral or central, and cause them to function automatically as different groups of ideas. If the threshold were sufficiently low, it might become "co-conscious," without entering the field of personal consciousness. This co-consciousness is really conscious because it behaves so, being able to solve problems, and because it says it is conscious. For Prince there is no normal "subconscious or subliminal self or hidden self."

Janet[1] who likewise limits "subconscious" to the pathological co-activity of divided personality, thinks there may be a group of co-existing ideas in the normal individual because "pathological phenomena have their germ in normal physiology." This aggregation is due to weakness in power of synthesis. In hysteria the power of psychic synthesis is so weakened and consciousness so narrow that when one per-

[1]Janet, P.: A Symposium on the Subconscious. *Jour. Abnormal Psy.*, 1907-8, II, p. 62.

ceives an impression he is inaccessible to others. Sometimes he is unable to receive impressions from more than one sense realm, or he may even be able to obtain data from impressions from one side of the body only. Ideas are not associated with one another as with normal people, but every idea takes up the whole narrow activity of consciousness.

Sidis, on the other hand, considers a sub-conscious self the possession of every individual normal and abnormal. The subconscious is not an unconscious physiological mechanism, it is a secondary consciousness, a secondary self.[1] It may possess some degree of self-consciousness, may grow and develop. "As a rule the stream of sub-waking consciousness is broader than that of waking consciousness, so that the submerged, sub-waking self knows the life of the upper, primary, waking self, but the latter does not know[2] the former. This self is manifested by all the facts of "crystal gazing," "shell hearing," automatic writing and the like. They "reveal the presence of a secondary, submerged, hyperæsthetic consciousness that sees, hears and perceives what is outside the range of perception of the primary personal self." This sub-awaking[3] self shows itself present in post-hypnotic suggestion, "shell-hearing," "crystal vision," etc. It is extraordinarily plastic and devoid of all personal character. The subconscious is by no means identical with states of low intensity, but includes psychic states ranging from the lowest to the highest tension and vividness of mental activity.[4] In the functional relation of nervous elements he finds the physiological basis for the disaggregation of consciousness. The neurons form combinations of ever increasing complexity, and the more complex their organization the greater the organization of psychic units into systems. The individual mind is therefore a complex system of many minds. "There may be as many different personalities, parasitic or secondary, as there are possible combinations and disaggregations of psychophysiological aggregates." A neuron aggregate, entering into association with other aggregates and being called into activity from as many different directions as there are aggregates in the associated cluster, has its neuron energy kept within the limits of the physiological level. A dissociated neuron aggregate, on the contrary, is not affected by the activity of the other aggregates; it is rarely called upon to function and stores up a great amount of neuron energy,—

[1] Sidis, B. and Goodhart, S. P.: Multiple Personalty, N. Y., 1905, p. 128.
[2] *Ibid.*, p. 138.
[3] *Ibid.*, p. 45 and 184.
[4] *Ibid.*, p. 45 and 184.

with the equilibrium of the neuron aggregates, with the synthesis of the dissociated systems, the subconscious eruptions, attacks or 'seizures' vanish never to return.[1]

The explanation of Breuer and Freud for like phenomena is similar to that of Sidis. In some cases we find that "great complexes of ideas and complex psychic processes, rich in consequences, remain completely unconscious in many patients and coexist with the conscious psychic life."[2]

Cleavage is usually caused by the suppression from consciousness of a painful experience. Ideas producing the hysterical phenomena, though of long standing, are lively and actually present, their continued liveliness being due to a dearth of associations and external impressions. Cure consists in associating the suppressed experience with the rest of consciousness, for when an emotion is denied expression in reaction the intra-cerebral excitation is greatly increased but used neither in motor nor associative activity, and in some cases abnormal reactions enter and there is an "anomalous expression of the emotional life." When the complex is associated with other neural complexes, its excess energy discharges itself. Phenomena of daily life show the repression of painful memories and evidences of the effect of unconscious ideas, such as forgetting good resolutions or the return of a desired but borrowed book, and many symptomatic and accidental acts. Unconscious motives determine many of our actions. Dream work is a complex thought structure formed in the daytime and not discharged, leaving a remnant which persists and would disturb sleep were it not converted into dreams.

Jung[3] finds longer reaction times when the stimulus word is associated with an idea complex possessing a strong feeling tone. This complex, momentarily separated from consciousness, exercises an effect which concurs with the ego-complex. The "constellating" of an association is mostly unconscious, the complex playing the rôle of a quasi-independent existence, a "second consciousness."

GENERAL DISCUSSION OF THE ABOVE MENTIONED CONCEPTS

The above are the representative uses of the terms "conscious," "subconscious" and "unconscious," with the disputes therein involved. Objections to speaking of "unconscious psychic" processes are mainly on the logical ground that un-

[1]Sidis, B.: Psychopathological Researches, N. Y., 1902, p. 212.
[2]Breuer and Freud: Studien über Hysterie, Leipzig, 1895, p. 194.
[3]Jung: Ueber das Verhalten der Reactionszeit beim Assoziationsexperimente. *Journal f. Psy. u. Neurol.*, VI. 1905. 29. (Republished in C. G. Jung's Diagnostische Assoziationsstudien, Leipzig, 1906, I. 221 ff.)

conscious and psychic are contradictory terms. Metaphysical considerations on the other hand are responsible for the contrary opinion, for it is held that the law of universal causation must hold for the mental as well as the physical world. Changes often occur in consciousness with no consciousness, *i. e.*, with no consciously observable cause; therefore they must have an unconscious mental cause. The problem reduces therefore to one of epistemology;[1] the identification of consciousness and the psychic or the extension of "psychic" to cover the accompaniment of all material changes. As Hellpach points out, the unconscious can never be discovered by investigation, but only by hypothesis, by analogy, or metaphysics. He who denies the unconscious retreats to the empirical and has to explain all from the conscious side in which there are vast gaps. If he makes any assumptions he must say that consciousness causes physical changes and these again conscious ones, which is the interactionist's position, or that physical and mental changes are parallel, which is the position of either monistic or dualistic parallelism.[2] The attitude one takes reduces, therefore, ultimately, to a question of his temperament. Practically, it makes little difference whether one assumes the changes going on without consciousness but later affecting it to be complex neural changes only,[3] or changes possessing the conscious character in structure but lacking the conscious quality, or a psychic reality accompanying all existence, or a psychic accompaniment of molecular changes different in degree from those underlying conscious experiences.

Let us consider somewhat more fully the use of the terms "unconscious psychic process" in this metaphysical sense of the psychic accompaniment of physical processes lacking the conscious quality. By consciousness we mean that indefinable ultimate best described as experience or awareness. It is not identical with self-consciousness, which is only consciousness of one's past states of consciousness, immediate or remote. Consciousness is the broader term. Conscious states, however, are those which can become self-conscious later. Consciousness is always more or less complex, the elements entering therein contributing to the character of the whole, which is qualitatively different from these elements. It is the *interconnection* of the psychic processes, *i.e.*, it is the *association* of the elements constituting it. Where association is

[1] Münsterberg, H.: Symposium on the Subconscious, *Jour. Abnormal Psy.* II. 1907-08, p. 28.

[2] Hellpach: Das Unbewusste, *Central. f. Nervenheilkunde und Psychiatrie*, 1908, Bd. 31, S. 65-6.

[3] Münsterberg; Psychotherapy, N. Y., 1909, p. 140; p. 147.

strongest consciousness is most intense. We assume the existence of a psychic side concomitant with all material changes, both conscious and unconscious. "Unconscious" we use as diametrically opposed to "conscious," "psychic process" in the metaphysical sense of the psychic side of a process possessing a concomitant physical side. Such a psychic side we conceive to be present in all organic matter, if not in all matter. With the activity of each neuron, therefore, there is psychic process, but consciousness probably does not occur until there is a complex functioning of neurons in one system or pattern. The pattern may change and shift in its organization, now dropping out some elements now taking up others, but the whole is usually in a more or less close functional connection. It is however conceivable that two or more different complexes may be functioning with sufficient intensity to give different alternating or simultaneously existing streams of consciousness. Association of an aggregate with the personal aggregate is probably necessary for consciousness — in other words, association of elements with the general bodily sensations and feelings which constitute the fundamental part of our personality—what Bleuler probably means by "the association with the ego-complex."

According to this view ideas out of consciousness do not become physiological processes any more than they were such before. We must assume some disintegration of the neuron aggregates underlying the idea and with this disintegration some change in the idea itself, according to which it no longer possesses its former character, but is the psychic accompaniment of the physiological process.

As for the objection to the term "unconscious psychic process," we agree with Lipps[1] that every psychic *process* is unconscious. All that is given in experience is each separate state of consciousness, the process underlying the sequence of ideas or feelings never being a matter of consciousness, but something which is merely inferred. The inspirations of the poet and the associations of the genius are not more spontaneous than those of the ordinary man, only richer and more varied. The ordinary man may be able to trace and explain the sequences better than his more fortunate brother, but the processes underlying them are none the less unconscious.

As for "free-rising" ideas, or "mediate" associations, conscious connective links, rapidly forgotten, without doubt exist in many cases. When the idea can be traced to an association started from some external cause, of which the subject was absolutely unconscious, a peripheral physiological

[1]Lipps, T.: Leitfaden d. Psychologie, Leipzig, 1909, p. 83 f.

excitation has probably set into action a neuron complex out of connection with the main system, which is concerned in some other different state of consciousness. When this widens and the emphasis of activity shifts, the aggregate stimulated peripherally, without conscious, but with psychic, accompaniment may be taken up in the new arrangement of neuron complexes and become conscious. But sometimes an idea enters with no traceable connection. Here aggregates concerned in the "free rising" idea may be in a state of functional activity of a greater or less degree but separated from the centre of activity. Cells concerned but slightly in the conscious processes of the moment may set in activity others involved in the detached aggregate, which functions then with such completeness as to shift the centre of activity from the original to the new position, the new idea becoming the centre of consciousness.

The divided consciousness of multiple personality, and similar states sometimes experienced by normal individuals in dreams or abstracted conditions where consciousness seems to be divided into two alternating or coincident streams, is of the same general character as consciousness. The term "co-consciousness" is a better name for this experience than "subconsciousness," and is more definite in its meaning. Used in such a sense "subconscious" is too easily extended, even by the writer who so uses it, to cover something which is literally under consciousness, out of which consciousness arises and into which it descends. Used in such a way, it takes the place of the older concept of the "soul" as an independent creative entity. Just as the soul was responsible for our actions or looked out upon our thoughts, so its successor, the "subconscious self" is supposed to do.

Consciousness does not fade off from distinctness by ever fainter degrees into unconsciousness. Facts in the margin of consciousness are qualitatively the same as those in the focus, but the difference between the outer limits of the margin and the region beyond is absolute. "Subconscious," when used to denote the periphery of the conscious field, is a term descriptive of a condition of actual consciousness, different in degree; but as it is too easily extended to describe processes outside of consciousness, "perceptual" is a better term.

Analysis shows that expressions like "resting back on the subconscious" in prayer and meditation mean the relieving of mental tension by widening attention, so that activity can shift from newer, less firmly established association complexes to older, well developed complexes which have had survival value. The individual is "larger than his conscious-

ness,'' in that consciousness at best is so narrow as to embrace but a small part of the results of his own past habits and experiences, and those of the race seen in tendencies, appetencies or instincts. In such a sense any present experience is only to a small degree determined by conscious factors. One's motives for action are seldom clearly analyzed or made focal in consciousness. Oftener they are entirely without consciousness, being the results of past experiences and training which have developed characteristic modes of spontaneous response.

CONSCIOUSNESS IN ANIMALS

No objective proof of consciousness in animals is possible,[1] but the assumption of consciousness in them rests on inference, just as it does in our fellow beings, for the only place it can be positively known is in the individual himself. Denying the possibility of comparative psychology would therefore logically result in a like attitude toward human psychology.[2] Any objective criterion of consciousness must be arbitrary. ''Learning'' or ''modifiability of behavior'' as an indication of its presence is not good, for there is evidence that plants learn,[3] and even material[4] objects adapt themselves to repeated stimuli or changed conditions as the seasoning of a violin to strains of the master. There are also some indications that human learning goes on unconsciously.

III. The Relation of Consciousness to Learning

The relation of consciousness to learning has received some discussion as well as experimental testing. The problem of learning in general I have reviewed elsewhere[5] and shall consider here only the results bearing directly on the subject in hand.

THE LEARNING PROCESS

We may define learning as the formation of associations between certain stimuli and definite modes of reaction. The simpler and less varied the stimuli the simpler the learning process will necessarily be, and the more permanent the value of

[1]Yerkes, R. M.: Objective Nomenclature, Comparative Psychology and Animal Behavior, *Jour Comp. Neur. and Psy.*, 1906, XVI, p. 388.

[2]Claparède, E.: La psychologie comparée, est-elle légitime? *Arch. de psy.*, 1905-6, V. p. 34.

[3]Darwin and Pertz: On the Artificial Production of Rhythm in Plants. *Annals of Bot.*, 1903, XVII, pp. 93-106.

[4]Washburn, M. F.: Animal Mind, N. Y., 1908, p. 33.
Claparède, E.: The Consciousness of Animals. *Internat. Quart.*, 1903-4, VIII, pp. 296-315.

[5]Ellison, L.: The Acquisition of Technical Skill, *Ped. Sem.*, 1909, XVI, pp. 49-63.

the associations formed, the simplest form probably being that in the case of lower animals where the association is between a single stimulus and a simple movement, the highest and most complex form in man, where complicated movements must often be preceded by a train of thought possessing little motor accompaniment. The simplest form of learning involves, then, direct motor response to a simple stimulus, the highest, however, lacks much of this motor element, being for the most part an association of symbols, such as the growth in meaning of words, and the power of generalization from previous experience. Most learning involves both these factors.

Observation and experiment goes to show that learning to meet any new situation involves a specialization and perfection of some part of an already existing habit or mental possession. As Morgan points out, effective consciousness[1] finds itself a partner in a "going concern." The performance of the instinctive act whose co-ordinations are hereditary, and the consciousness such a performance evokes, are simultaneous.[2] The behavior and the conditions producing it occupy consciousness, but "the effects of the behavior, as the animal becomes conscious of the acts concerned, serve to complete and render definite the conscious situation. Consciousness, however, probably receives information of the net results of the progress of behavior and not of the minute and separate details of muscular contraction."[3] As Sherrington puts it, "the controlling centres can pick out from some ancestrally given motor reaction some part of it so as to isolate that as a separate movement, and by enhancement this can become a skilled adapted act added to the powers of the individual."[4] When a new movement is initiated an excess of energy is expended and with it occur many more or less random movements; of these, as the effort is repeated, a special movement, or a special series, finally stands out from the scattered mass. The clearer its separateness from the rest, the more vivid its conscious accompaniment and the power of conscious control. Consciousness of the way a movement feels is necessary for its voluntary performance, hence, as Judd's[5] experiments show, an abstract idea cannot

[1]Morgan, C. L.: Introduction to Comparative Psychology, N. Y., 1906, p. 51.
[2]*Ibid.*, pp. 99, 101.
[3]*Ibid.*, p. 105.
[4]Sherrington, C. S.: Integrative Action of the Nervous System, N. Y., 1906, p. 389.
[5]Judd, C. H.: Practice and its Effects on the Perception of Illusions, *Psy. Rev.*, 1902, IX, pp. 27-59.

take the place of direct perceptual experience. The way to get control of a movement as the experiments of Bair[1] and of Swift[2] show, is by working outward from some general movement over which we already have control. In Bair's experiment on learning to move the ears, the subjects began with the muscles over which they had conscious control, such as raising the brow, clinching the teeth, making more and more strenuous effort to get closer to the ear, an excess of motor energy being discharged with proximate muscles. "As soon as the sensation arising from the movement of the ear was associated with the concomitant sensations of muscles close to it, over which there was already voluntary control, there was a basis for learning the voluntary control of the ear." The definite idea of the movement given by electrical stimulation of the *retrahens* muscle was not sufficient to produce the movement, but it gave a general idea as to the direction the innervation was to take. As control developed attention was narrowed down from the general sensation of the adjacent muscles to that of the specific movement sought for. Likewise, in the control of the reflex wink, Swift found it necessary to begin with the muscles around the eyes over which there was conscious control. What Bair[3] says in regard to the general ability given by special training, *e. g.*, "to a new situation we react by a general discriminative reaction and are more likely to hit on a favorable response than without this special training," is true of all learning. For no matter what new acquisition is undertaken, if it is possible to master it, some previous general training has either been developed by the individual or through the inherited co-ordinations of his ancestors. Experiments on acquisitions of a more complex kind show the same fact—attentive consciousness cannot be directly and advantageously applied at first, because of the multiplicity of details which overwhelm it. The new experience calls up too many old associations which are not pertinent. Such facts account for the rapid rise of the learning curve at first, when responses are selected from a mass of older habitual reactions, and its slower ascent later, when associations really new are being formed.

CLEAR CONSCIOUSNESS AND LEARNING

The importance of clear consciousness in learning is shown by the following facts. Experiences causing greatest atten-

[1]Bair, J. H.: Development of Voluntary Control, *Psy. Rev.*, VIII, p. 499.
[2]Swift, E. J.: Studies in the Physiology and Psychology of Learning, *Am. Jour. Psy.*, 1903, XIV, pp. 200-251.
[3]Bair, J. H.: The Practice Curve, *Psy. Rev., Mon. Sup.*, 1902 V.

tion are best remembered. Desire to succeed and intense effort are necessary for progress, which means that one must attend closely to the matter at hand. Even in learning of a purely muscular sort, where attention to the movement itself has been found to be a hindrance, attention to the objective features of the task is required for the perfection of the unconscious or dimly conscious part of the reaction. The fact that subjects of a given mental type are most interfered with in their learning, by distractions appealing to that type of imagery, shows that undisturbed consciousness is essential. Trying to recite a syllable series is more effective in establishing the syllables than merely reading them,[1] because of the narrower attention required. Figures drawn with the left hand are better remembered than those drawn with the right, for the same reason;[2] the greater ease of remembering sense material as compared with nonsense is also probably due in part to the easier application of attention.

Results from experiments on cross education point to like conditions, for the more similar the training and the test material the greater the transference.[3] Improvement consists in more economic methods of work,[4] and is essentially "attention training." Transference consists in the carrying over of right hand "methods" to the left hand. Perhaps the most adequate study of this problem is a recent one by Fracker,[5] who finds that the most essential element in transference is imagery and that improvement occurs if imagery is developed in the training series which can be transferred and advantageously used in the test series. It may be subconsciously developed, but if it comes to be consciously recognized, the improvement is more rapid. "The rate of improvement seems to depend directly upon the conscious recognition of the imagery and upon attention to its use. The transference of elements is a conscious transference." Improvement during intervals of no practice seems to be due in part to freshness and better attention, in part to the fact that interfering habits are forgotten, so that better and more

[1]Witasek, S.: Ueber Lesen und Rezitieren. . *Zeits. f. Psy.*, 1907 Bd. 44; pp. 161-185; 246-282
Katzaroff, D.: Expériences sur le rôle de la récitation, *Arch. de Psy.*, 1908, VII, pp. 255-8.

[2]Rowe and Washburn: The Motor Memory of the Left Hand, *Am. Jour. Psy.*, 1908, XIX, p. 243.

[3]Ebert u. Meumann: Ueber einige Grundfragen der Psychologie der Uebungsphänomene, *Arch. f. d. ges. Psy.* 1905, IV, S 1—232.

[4]Swift: *Op. cit.*

[5]Fracker, G. C.: On the Transference of Training in Memory, *Psy. Rev.*, Mon. Sup., IX, 1908, 56-102.

practiced ones may be free to assert themselves.[1] Then, too, attention is not distracted by the new elements of the situation, but can be more economically applied. The value of clear consciousness in learning is that it assists the selecting of good elements from the complex reaction and the eliminating of disadvantageous factors. When the subject is weary he is apt to fall into bad habits which are more difficult to modify because unconscious.

Consciousness of details and elements of a process gradually gives place to consciousness of larger and more complex difficulties. These elements gradually form themselves into larger wholes and consciousness works with greater units. This is true not only of muscular learning, but of more intellectual activities such as typewriting, the telegraph language and chess. As Cleveland[2] puts it, with reference to chess, "learing requires the perfection of the elements and their organization into ever larger groups, so that attention is not bound to details, but left free to forge ahead and anticipate difficulties." In the writing processes, first letters, then words, then sentences are grasped. In chess one grasps the situation by larger and larger wholes. Cleveland puts it thus, "Progress in chess consists in the formation of an increasing symbolism which permits the manipulation of larger and larger complexes. . . . There is something in the purely intellectual life corresponding to motor automatism, which is shown in the ability to think symbolically or abstractly, and thus to handle large masses of detail with a minimum of conscious effort. It involves the increasing ability to take in during a single pulse of attention a larger and larger group of details which means, of course, that the attention is no longer needed for each one."

The importance of the motor element in learning verbal material is without doubt due to clearer consciousness of the task, the material appealing not only to vision, but to hearing and the kinæsthetic senses.

SUBCONSCIOUS AND UNCONSCIOUS LEARNING

Such are some of the observations as to the rôle of consciousness in learning, but much of our learning goes on below consciousness. As Kuhlmann[3] points out, much of our most important learning—the use and functional activity of our own

[1]Book, W. F.: The Psychology of Skill, U. of Montana Bull., No. 53, 1908, p. 6 ff.

[2]Cleveland, A. A.: The Psychology of Chess and Learning to Play it. *Am. Jour. Psy.*, 1907, XVIII, pp. 269-308.

[3]Kuhlmann, F.: The Place of Mental Imagery and Memory among Mental Functions, *Am. Jour. Psy.*, 1905, XVI, p. 337-356.

bodies—goes on with no conscious direction. The digestive apparatus must learn to do its work, and so must other internal organs Early reflexes such as the reflex wink and mimetic expressions, binocular vision, the co-ordination of voluntary muscles, develop without *voluntary* use of incoming stimuli for their guidance.

The very possibility of learning rests on an unconscious basis of physiological endowment. The dullard and the genius are alike dependent upon their physiological inheritance, and the quick wits of the healthy child are as much beyond his conscious control as are those of the feeble defective.

We are not conscious of the "process" underlying our associations, but merely of the results as they present themselves to consciousness. In fact learning may progress without our knowledge of the fact, as is seen in the development of unconscious automatisms. For example, one may develop peculiar manners of gait or expression without knowledge, having unconsciously imitated some one possessing a like peculiarity. Through the ever present suggestions of a new environment we may develop new ideals and new apperceptive attitudes of which we are unconscious until we are taken back to our old surroundings. In learning of a muscular sort we may have been conscious of every sensation leading up to the subsequent habit, without consciousness of the method in which we work, or of the existence of the habit itself. An example is given by Pfungst.[1] Having directed his subjects to think of one of two similarly sounding words of a series to which he would respond with certain arm movements, he was able to tell by the direction of the head or eye movements, of which word they were thinking and to which they expected him to react. By changing his manner of responding he obtained a similar change in their movements. He was also able to tell by head movements of which direction, left or right, his subjects were thinking. He concludes that "the changing of natural movements of expression and the acquisition of new ones are both possible without knowledge of the person."

Our environment is one of the strongest factors in training us, whether it operates consciously or unconsciously. One hears good language continuously, and easily forms the habit of using it himself. The development of our ethical ideals and æsthetic feelings and our very forms of thinking unfold before we are conscious of their existence. Such habit formation rests on instinctive imitation and forms one of the most important classes of learning.

[1]Pfungst, O.: Das Pferd des Herrn von Osten, Leipzig, 1907, p. 77 ff.

Learning may also progress without consciousness of the end or purpose. Such a fact has its best illustration in the instinctive activities of animals and the play of children and animals. Play is a training process for life where most of the activities required in later life have their initial, though unwitting, development.

The formation and strengthening of associations below consciousness is indicated by some of the laboratory experiments on learning. This must be the explanation of the fact that scattered repetitions, which deal with older associations, give best results. Müller and Pilzecker think the greater strength of the older associations is due to the tendency of an excitation to outlast the stimulus—to a "perseveration tendency"—as a result of which an idea rises of its own accord into consciousness without associative connections. Illustrations other than those found in learning nonsense syllables are the following: The histologist's illusion which occurs after working long and intently with the microscope; images seen with closed eyes often have then the character of the microscopic forms. Similarly when one studies or thinks intently of any subject, carelessly perceived objects tend to take on its character. While studying the anatomy of the internal ear every gas jet or twisted twig was a cochlea, for the writer. It is not uncommon to see, before sleeping, scenes which have passed before the eyes while travelling, or on a tramp. "Crystal vision" may illustrate the same phenomenon, freeing the mind for the appearance of ideas underlying which is this perseveration tendency. This fixing of the association probably goes on physiologically whether the ideas crop out into consciousness or not, for Müller and Pilzecker found that attention to any other engrossing matter prevented their recall, although free reproduction was not a tendency with all their subjects; nor was the hindrance due to preventing the subject from thinking over the series. They conclude that "after the reading of a syllable series, certain physiological processes which serve for the strengthening of the associations formed by the reading of a series continue for a certain time with gradually diminishing intensity."[1]

"Retroactive amnesia" or the forgetting, after a shock, of incidents extending backward from the shock to several hours or more, likewise points to the probability of the physiological fixing of associations.[2]

Experiments on the acquisition of skill show that unconscious habits are developed which consciousness either selects

[1]Müller u. Pilzecker: *Zeit. f. Psych.*, Ergänzungsband I, 1900. p. 196.
[2]Burnham, W. H.: Retroactive Amnesia. *Am. Jour. Psy.*, 1903, XIV, p. 386-7.

or represses. As Swift says, "Consciousness discovers certain methods in operation and approves or disapproves them."[1] Subjects improve by "hitting upon" better ways of working, without any further conscious selection, at first, than the general effort to succeed. Book's experiment shows the same thing. "A mass of old associations are called up, only a few of which are directly serviceable for the work. From these there are unconsciously built up, by the double process of elimination and selection and reorganization, the first elementary associations (letter associations) used, and from these in turn the later, higher order habits. There comes to be . . . a sort of unconscious struggle for existence among the many modes of action, ending in the survival of the one direct and economic way of reaching the goal desired."[2]

Automatization of elements previously conscious occurs in the perfection of all activities, leaving consciousness free to undertake more difficult features.

IV. An Experimental Study of the Relation of Consciousness to Learning

Our own investigations undertook to discover by experiment (1) whether learning is helped by factors which never come into consciousness, or are present only to a minimal degree, (2) whether the formation of a habit of whose existence and development one is unconscious can progress as well under distraction, when consciousness is removed as completely as possible from all the elements which go to make up the habit formation; and finally, to find the rôle of consciousness in learning simple tasks involving, (3) almost no intellectual factor, (4) a complex co-ordination of muscular impluses, and (5) learning of purely intellectual character.

1. *Do Unnoticed Items Assist in the Formation of Associative Links?*

Experiment 1 was suggested by Scripture's experiments on the associative course of ideas,[3] and work of a similar sort,[4] which seemed to show that unnoticed features of a total impression (like an inconspicuous Japanese symbol or a numeral placed beside a word or a picture) could serve as a bond to connect the given word or picture with another word or picture which had been elsewhere accompanied by the same symbol or numeral. The theoreti-

[1]Swift, E. J.: Studies in the Psychology and Physiology of Learning. *Am. Jour. Psy.*, 1903, XIV, p. 201-251.
[2]Book, W. F.: Univ. Montana Bull., No. 53, 1908.
[3]Scripture: *Op. cit.*
[4]Sidis: The Psychology of Suggestion, N. Y., 1898, p. 171.

cal importance of the question and the fact that most attempts to repeat Scripture's work had led to negative results invited a new attack. The plan which we undertook may be illustrated by the following scheme, though the actual execution of the experiment was carried out with greater refinement and in a different way as to details.

The observer is presented with a triple series of meaningless syllables, as in Group I below, and is required to read series *b* a certain number of times and if possible learn it. Series *a* and *c* are of course all the time before his eyes though not involved in his task. After reading *b* the required number of times, his knowledge of it is tested by the "Treffer method," and his success in giving the required syllables recorded. Then after a brief interval he is presented with Group II of which the middle series is the same as one of the side series in Group I, *e. g.*, series *a*, and he is required to read (and learn) series *a* in the same manner in which he has just read (and learned) series *b*.

Group I

a	*b*	*c*
jex	mil	peb
yal	hud	yom
bup	gib	lur
dof	dep	zal
tem	voz	bic
fuj	pog	vop
nen	lek	loh
gop	gaj	nat
riz	fiv	jof
mod	yem	wam

Group II

d	*a*	*e*
miv	jex	hal
sem	yal	juj
coj	bup	pom
fet	dof	das
dib	tem	lef
vil	fuj	roj
buj	nen	zup
hix	gop	fab
kug	riz	uls
len	mod	veb

If *a* is on the average learned with greater ease or completeness than *b*, the inference is that the previous presentation of *a* in indirect vision has somehow been helpful—directly by rendering the syllables individually more familiar, or indirectly through their association with the syllables of series *b* which have in the learning been associated with each other. If *a* is not learned on the average more easily or perfectly than *b* the inference is either that no assistance is gained by the "unconscious" perception of *a* or that the gain is not of sufficient amount to be determined by this method of experimentation.

Such experiments were carried out on two trained observers through a considerable number of days, but it may be said at once that the results were on the whole negative. There was no clear evidence of any advantage. The presumption is that the assistance gained is small in amount—too small to be deter-

mined by this method. Later experiments undertaken expressly to determine the delicacy of the method showed that one reading of the *a* series with full attention had no beneficial effect upon the learning of the *a* series after an interval of ten minutes during which the *b* series had been learned.

Though the results of this series of experiments must therefore be set down as inconclusive, they may have, perhaps, a certain value in other connections and are therefore given in the Appendix of this paper.

2. *The Effect of Attention and Distraction on the Formation of the Motor "Set" (Motorische Einstellung)*

EXPERIMENT 2. The purpose of the second series of experiments was to find the effect on the "*Motorische Einstellung*" of attention and distraction. The term "*Motorische Einstellung*" indicates the effect which repeated lifting of a heavy weight has in making subsequent lighter weights seem too light. It is probably due to a temporary habit of the nervous system. The problem in our case was to discover whether a neural habit of this sort, of whose existence the subject was unaware, would be more readily formed when he was attending to the lifting of the heavy weight than when he was inattentive to it.

The phenomenon of "*Motorische Einstellung*" was first reported by Müller and Schumann.[1] They lifted a moderate weight of, say, 600 grams and, after it, lifted a heavier weight of 2,400 grams to an equal height a certain number of times, in a definite rhythm. Then a weight of 800 grams was lifted and found to seem lighter than the 600 grams, lifted before the training with the weight of 2,400 grams. They explain the illusion by saying that the 800 grams, which is lifted with an unusually powerful impulse after the work with the weight of 2,400 grams, rises with unusual speed and therefore seems lighter than the first weight, because we are apt to judge as lighter a weight which raises more quickly. The repetition of the lifting of the heavy weight has set up a tendency in certain sub-cortical centres to discharge automatically with a somewhat extra intensity. Experimentation of this kind was carried further by Steffens.[2]

The apparatus used is pictured in the accompanying cut. Two boards measuring about eighteen inches long were clamped to the sides of the bottom of a chair so that the ends

[1]Müller u. Schumann: Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, *Pflüger's Archiv*, XLV, 1889, 37-122.

[2]Steffens: Ueber die motorische Einstellung. *Zeits. f. Psy.*, Bd. 23, S. 240-308.

extended about seven inches beyond the front edge. Holes were bored near the forward ends of the boards and through these were passed the ends of two handles by which the weights were lifted. The upper parts of the handles were made of wood and were provided with grooves into which fitted the fingers of the observer, enabling him to hold the handles firmly and in the same way each time he lifted. The handles below the board consisted of brass rods having at their lower ends disks of wood, on which the weights rested. An iron needle was passed through each brass rod in the middle, making it possible to raise the handles only a given distance. To prevent the needles hitting against the boards with a jar, a string was fastened in front of the chair, by means of two iron standards clamped to the table, at such a height that the observer's hands would touch the string before the needles came in contact with the boards; as soon as the hand touched the string the weight was lowered. A disk of cork was used on each handle to prevent the clinking of the weights against each other. The entire weight of each handle with the cork disk was 100 grams.

The chair stood on one of the large laboratory tables. As far as possible from the observer a metronome was placed, its noise being deadened by a cloth pad between it and the table. The experimenter sat at the side of the table to the observer's left, and changed the weights as the experiment required. These were flat and circular in form with a rather large slit so that they would slip on and off the handles easily.

The method of the experiment was this: The right-hand weight was always the standard, and was always kept at 300 grams, *i. e.*, a 200 gr. weight plus the weight of the handles. By trying different weights a weight was found for the left hand which usually seemed equal to the right-hand weight. Owing to the difference in strength between the right and left hands this was actually a weight much smaller than the standard. Since practice was apt to increase the strength of the left hand, it was necessary to determine what this weight was before every experiment; and doing this counteracted also any influence which might have been carried over from lifting heavy weights in the experiment of a previous day. After determining the apparently equal weight, twenty judgments were made, upon weights offered for comparison with the standard (300 grams in the right hand), four with the weight which had been judged equal, and four each with weights ten and twenty grams above and ten and twenty grams below the "equal" weight. If the judgments were perfect the results would of course show four judgments "equal," eight "heavier" and eight "lighter." As a matter of fact they

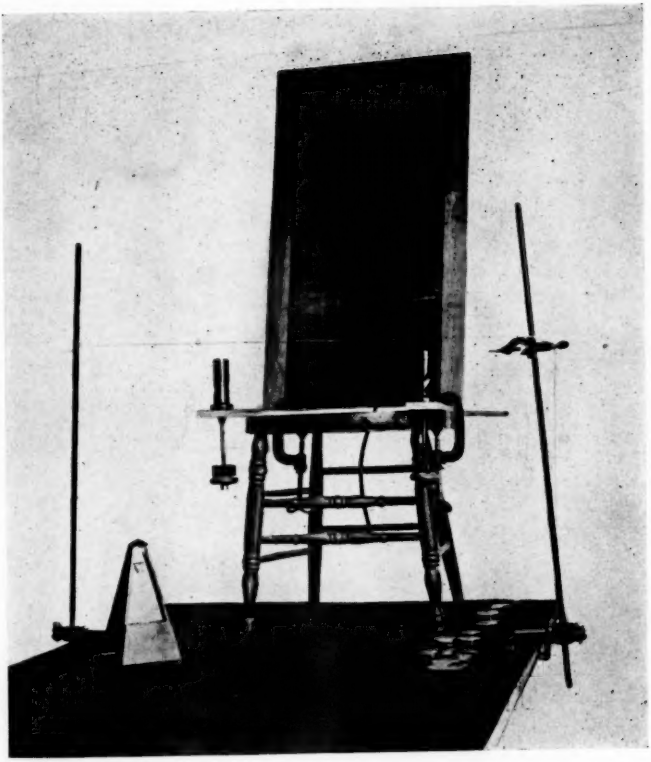

Fig. I.

varied a little, this way and that, as is common in such experiments. The lifting was done rhythmically to the stroke of the metronome, which beat at about four per second for the different observers, some requiring a slower rhythm than others. The rhythm was constant, however, for each subject. At "one" the standard was raised, at "two" lowered, on "three" the comparison weight was raised and on "four" lowered.

The results of the twenty lifts just described served as a basis of comparison for similar lifts after a period of lifting a heavier weight to establish the *Einstellung*. This heavy weight, which was called the "training" weight, was for all subjects a weight twice as great as the one which had seemed equal. The experiment here differed a little in its method for the lifting "with attention" and "with distraction," though the actual weight lifted and the rhythm for lifting were the same for each. In the experiment "with attention," the subject was told that he must determine, by estimation, a weight twice that of the standard, and that he would be given two weights above and two weights below this double weight, and sometimes the double weight itself. He was to raise the standard just as he had done in the previous judgments of equals, which would aid him in his judgment, and he was to judge as quickly as possible. Here the standard was raised on "one," lowered on "two," a pause on "three," the variable raised on "four," lowered on "five," and the judgment given immediately. While the judgment was being given the experimenter shifted the weights, and the subject began to raise the standard in nearly all cases after the sixth count. The method of lifting was at first not carried out in this three-six rhythm, but in a two-four rhythm; but was changed after three days of experimentation to make the rhythm the same for the experiments "with attention" and "with distraction." All the results are incorporated in the tables which follow, as the change of rhythm seemed to have no disturbing effect. Ten judgments were made, four of the weights being greater than the "double" weight, four less, and two the "double weight" itself, so that the actual weight lifted amounted to lifting the double weight ten times. Immediately after giving the tenth judgment the five original (*i. e.*, nearly "equal") weights were compared with the standard, to test the *Einstellung*, and the results recorded. After a rest of from one to two minutes the double weight was raised five times more, in the manner just described, to refresh the *Einstellung;* and immediately after, the original (nearly "equal") weights were again compared. This was done until the training weight had been raised twenty times in all and the original weights had been compared twenty times. In schematic form the ex-

periment was: (1) to determine a weight which when lifted by the left hand should seem equal to the standard weight, when lifted by the right hand. This was done by offering the original weights in such a way that the "equal" weight was raised four times, the ten and twenty grams "heavier," four times each, and the ten and twenty grams lighter, four times each. (2) Ten lifts of the training weight, followed by five comparisons of the original weights arranged according to a regular permutation. (3) Five lifts of the training weight, with five judgments of the original weights. (4) Repetition of (3). (5) Repetition of (3).

The experiment "with distraction" differed from that with full attention only in so far as lifting the training weight was concerned. Here the "double" weight only was lifted ten times, with a pause, then five times with a pause, and so on, until twenty lifts had been made; the actual weight lifted, however, amounted to the same for both forms of the experiment. While the observer was lifting the training weight in the "distraction" experiment, the experimenter read as distinctly as possible from some interesting reading matter. After the lifting of the original weights, which immediately followed that of the training weight, the subject was asked to give the content of what had been read, and a record was made of his success.

Four university students of psychology, two men and two women, served as observers. The number of experiments per observer varied from 14 for observer I, to 28 for observer IV, owing to modifications made necessary by the differences of the individual observers. Each experiment represents twenty lifts for the original weights, twenty for the training weight, and twenty again for the original weights.

The *Einstellung* was present unmistakably in the case of each observer. Observer I showed a clear and decided difference in the intensity of the *Einstellung* under the two conditions of the experiment, the effect being much greater when the training weight was lifted with attention than where distraction was used. This is true for the total and also for all except one of the single pairs of days on which experiments with attention and with distraction were made. The other observers, however, showed this difference but slightly or not at all. The difference was so clear for observer I that an explanation was sought for the indefiniteness of the records of the other three observers. As none could be found at first for observer IV, his work was continued until 18 complete experiments had been performed. The difficulty was discovered for observers II and III and the experiment modified after 12 complete experiments. The following table gives the

results obtained from the work just described. The first column represents the observer, the second the number of

TABLE I

		No. of lifts	Ra		Ta		Rd		Td		Dif. a		Dif. d		a—d	
			L	E	H	L	E	H	L	E	H	L	E	H	L	E	H
I	Obs.	140	49	50	41	113	21	63	79	45	16	+64	−29	−35	+41	−11	−30	+ 23	−18	−5
II		140	50	36	54	108	22	104	103	20	7	+58	−14	−44	+54	−20	−34	+ 4	+6	−10
III		120	42	36	42	99	13	81	96	22	2	+57	−23	−34	+55	−8	−37	+ 2	+3	− 5
IV		180	48	85	47	105	67	84	107	64	9	+57	−8	−39	+64	−24	−40	− 7	+6	+1

lifts, the third (*Ra*) the results before training on days when the training weight was lifted with attention, the fourth (*Ta*) the results after training on days when the training weight

was lifted with attention. Column *Rd* contains the results from the lifting of the original weights before training on the days of the "distraction experiment," and *Td*, those obtained after lifting the training weight on the same days. *Dif. a* shows the difference between the results before and after the training weight, in other words the amount of the *Einstellung*, for the experiments with attention. *Dif. d* gives amount of the *Einstellung* for the distraction experiments. Column *a—d* gives the difference between the *Einstellungen* in the two cases. L, E, H, stand for "lighter," "equal," and "heavier" cases respectively. It will be noticed that some superiority of the *Einstellung* with attention exists over that with distraction except for observer IV, in whose case the opposite is true.

The *Einstellung* came out so clearly in both forms of the experiment with observers II and III, that it was thought that the training weight was so heavy as to give a tolerably intense *Einstellung*, irrespective of attention and distraction. It was therefore reduced a hundred grams to make the *Einstellung* more moderate, and the greater effect "with attention" immediately showed clearly against that "with distraction." The explanation for observer IV was different, and was obtained by an examination of the columns giving the results before the training weight had been lifted (columns *Ra* and *Rd*). The equal cases should number something near one-fifth of the total number and the heavier and lighter about two-fifths each; but the proportion is almost reversed, showing that weights just above and just below the "equal" weights were not discriminated from the "equal." To remedy this difficulty the training weight was kept as before, but the original weights were decreased (and increased) from 10 and 20 grams below and above the "equal" to 20 and 40 grams below and above, in order to make the possibility of discriminating greater. His results then showed the same tendencies as the other observers. Table II gives the results. The lettering of the columns has the same significance as for the preceding table. The first line of figures for each observer reading across the table gives the results of the modified experiment. The second gives the combined results of the first and second form. Observer I, performing the experiment only in its original form, is represented by but one line of figures.

After the experiment was closed a final test was made with each observer both with distraction and with attention, but the observer was asked to notice his manner of lifting, and to see if it differed subjectively in either case. Observers I and II reported them the same. Observer III held the weights a little looser in the lifting with attention but the lifting itself was the same. Observer IV raised the weights a little more

TABLE II

	No. of lifts	Ra			Ta			Rd			Td			Dif. a			Dif. d			a—d		
		L	E	H	L	E	H	L	E	H	L	E	H	L	E	H	L	E	H	L	E	H
Obs. I	140	49	50	41	113	21	6	38	56	46	79	45	16	+64	−29	−35	−11	−11	−30	+23	−18	− 5
II	120	61	18	41	79	25	16	58	29	33	73	31	16	+18	+7	−25	+2	+15	−17	+3	+5	− 8
	260	111	54	95	187	47	26	107	69	84	176	51	23	+76	−7	−69	−18	+69	−51	+7	+11	−18
III	100	57	26	23	72	15	13	57	18	25	64	19	17	+21	−11	−10	−8	+ 7	−1	+14	−12	− 2
	220	93	62	65	171	28	21	98	58	64	160	41	19	+78	−34	−44	−9	+62	−45	+16	−17	+ 1
IV	100	21	33	46	49	32	19	36	28	36	52	32	16	+28	− 1	−27	+4	+16	−28	+12	− 5	− 7
	280	69	118	93	154	99	27	79	116	85	159	96	25	+85	−19	−66	−20	+80	−60	+ 5	+ 1	− 6

sharply and slightly higher "with distraction." This difference was too slight to be detected by the experimenter. Ob-

server IV was the only one who had any idea as to the purpose of the experiment, and he had surmised it.

Taking all facts into consideration it seems certain that with a training weight which gives a moderate *Einstellung* and original weights which are different enough in value to render discrimination easy, lifting the training weight with full attention produces a more intense *Einstellung* than lifting the same weight in the same manner, but with distraction.

3. *The Rôle of Consciousness in the Acquirement of Muscular Skill*

The third series of experiments was of a very simple character and useful chiefly in furnishing opportunity for introspection. It consisted in learning to throw balls at a target about eight feet in diameter, from a distance of 14 to 18 feet. Two university students of the psychological department served as observers. One, the writer, had almost no previous experience of the sort; the other, a gentleman, had thrown some, but "not enough to amount to anything." Ten throws were made in close succession, then a pause until the observer was rested, then ten more throws until fifty had been made. The experiment covered 16 days of 50 throws each, these days occurring, with few exceptions, in uninterrupted succession.

The experiment was not prolonged far enough to give a satisfactory learning-curve, such as has been found for similar work by other experimenters, *e. g.*, Bair, Book, Swift, and others, since general introspective results were the main object of our work. It may be said, however, that the greatest gain, both in uniformity and amount of score, came for each subject in the first few days.

In learning to throw at a target one must specialize and perfect certain elements of the complex mass of neuro-muscular co-ordinations of which he is in possession through inheritance and his own practice in general activities. His consciousness is taken up primarily with the target and the ball in his hand and vaguely with those particular and general bodily sensations which enter in to make up the "set" of the situation. Only gross errors, such as standing too far to the right or the left, or throwing with too great or too little force, are consciously corrected. The minuter, more skilful adjustments developed of themselves out of the larger, less perfect ones already existing, and were then perhaps consciously continued or avoided. Attention to the mechanical side of the throwing only resulted in inferior work; yet clear consciousness was necessary for good results, but it was consciousness of objective elements—the target and to a certain extent of the ball—rather than of

one's arm or its movements. After a little practice both observers mentioned the fact that greater concentration and fixation of attention on the target resulted in better throws. When greater effort consisted in deliberate attention to the *mark*, good throws resulted; but when, as occasionally happened, the observer tried to regulate the *process* and attended to the hand, arm, or ball, random shots were sure to occur. One observer remarked: "I don't believe thinking of the thing will do any good. All I can do is to stand before the target and *wish* to throw well;" the other said that he was simply trying to make good throws but did not know how he did it.

Good physiological condition, interest in the work and a tonic muscular condition seem to be concomitants of success, for when observers are ill they lack energy and interest, throw almost listlessly, and with poor results. On good days they stand erect with muscles tense and eyes fixed on the target and do their best work. One observer at such a time even found himself forcibly squeezing the ball. The amount of energy put forth grew more regular with practice, *i. e.*, there was better co-ordination.

The introspections show that, in such an almost purely sensory-muscular process, skill develops without consciousness of the details. The peripheral sensations accompanying or preceding the reaction contribute to form the background of consciousness and to produce feelings of satisfaction or dissatisfaction according as the movements are rightly or wrongly made. Consciousness has little place "as guide," save in the grosser features of the task, but attentive consciousness of the end was necessary for the development of these peripheral adjustments. Clear consciousness seemed to be accompanied by a general neuro-muscular tonicity favorable to the best work. Probably with clear consciousness the organism is acting more as a unit of closely knit parts, each of which is then more effective on every other part while it is active, than in a state of disintegration where association is loose.

4. Learning to Write in Unaccustomed Ways

The experiments of this series were, like the last, of a simple sort, though they involved skill of a somewhat greater complexity. They consisted in learning to write ordinary script with the left hand, and mirror script with both the right and left hands.

Left-hand Writing. In the normal script experiments eight observers assisted, four of whom were men and four women. All were trained psychologists, except one woman who, nevertheless, had had much practice as an observer and was

excellent at introspecting. One observer was almost ambidextrous, two were left-handed. No definite tests of mental type were made, but the indications are that two observers were, in this sort of work, predominantly motor.

The conditions of the experiment were kept as nearly constant as possible for each individual during all his work The experiment covered a period of about fourteen days. The standard sentence, written by all, was, "Motives are like chemicals. The more you analyze them the worse they smell." This the observer repeated several times before beginning to write in order to learn it. The sentence was written three times with the right hand with timing (with a stop watch); then once without, and three times with the left with timing and once without, with sufficient pauses between tests to avoid fatigue. It was explained to the observers beforehand that the timing was merely an incidental matter and that they should write at a convenient speed, merely writing each sentence continuously. After each sentence had been written, the observer was asked to give introspections as to methods used, points attended to, and any other items which might be of interest.

Inference as to the part "unconscious" factors play must rest partly on the fact that the observer fails to mention them and it is therefore open to the error of supposing that facts not remarked upon are unconscious, whereas the fault may be due to incomplete introspection or report. Yet it was impossible to ask definite questions as to position or methods, for then entirely unnoticed factors became clearly conscious and the subsequent course of procedure was apt to be changed. Exact objective measurements of improvement in writing are naturally, impossible, but must be judged in a rough way by greater uniformity in the slant and strength of the characters, and by their greater clearness and legibility.

The different observers manifested individual differences in their adaptation to the task, their methods of procedure and the speed and proficiency acquired; yet there are elements common to all. It is evident that easy and natural writing movements with the left hand cannot be made unless one assumes a position nearly symmetrical to the customary right-hand position and lets the hand take a free and uncramped movement. This will result in script with a "back hand" slant of a rather uniform character if one writes on a horizontal plane. It was to this position and writing that all observers tended, though they arrived at it in various ways and adopted it to different degrees.

Only three observers assumed an entirely symmetrical position from the start. Two of these, who were left-handed

observers, did so unconsciously, guided purely by the "feel" of the thing. The other observer analyzed the situation, discovered that this would be the proper way, and so took the position voluntarily. A fourth observer assumed a position almost symmetrical, and one which was little changed during the progress of the experiment. This he said he did "consciously and unconsciously," *i. e.*, semi-consciously. Of the remaining four observers all began with the paper in exactly the position used for the right hand, with the body turned at the same angle to the table and the left hand and arm twisted into an awkward position, the wrist cramped over to the right side of the body. With one observer the right hand held the paper at the upper left corner, taking the position which the left hand had always used, thus showing that each hand had changed places with the other.

All observers, save one, finally used some finger movements for the left hand, but only four began with them. Two of these were left-handed and one ambidextrous, and used the movements unconsciously. Observer V, who analyzed his position and assumed a symmetrical one consciously, used finger movements at first, but after the first sentence the natural tendency to use larger arm movements manifested itself, and the finger movements disappeared. One observer made them to get out of a difficulty and after that tried for them; another noticed her right hand carefully while writing, observed the finger movements, realized that skill could be obtained only if the finger movements were used in the left hand, and therefore consciously adopted them, but with considerable effort, and it was only when attention was directed to the hand that they were constantly made. Another observer "found finger movements coming of themselves" and continued them because the writing as a result was better, but even then they were hard to keep. Of the observers who did not use finger movements at the start, one adopted them on the second day, one on the fourth and one on the fifth. The only observers using finger movements naturally are those possessed at the start of some skill with the left hand.

Four observers went from a larger to a smaller hand, three to a slightly larger one, and for one, size remained about the same.

The large movements at the start may be due to one of two things. They may be the result of a general tensing up of all the muscles in the intense effort of the new occupation, and a general spread of energy over the whole body—a thing which could be observed in the tense muscles of the hand in five cases; in the digging and scraping of the pen in three cases; and in tension about the mouth, head-movements or

raising the heels from the floor, some of which were noticeable in all but the left-handed subjects. Or they may be due to the fact that the muscles involved in the larger movements have been trained in many daily occupations while the finer movements have been very little practiced. In the progress of the race one hand has been specialized for the more skilful work, the other hand (in most people, the left) being used far less. The left hand of an adult just learning to use it in left-hand writing, is in about the same condition as the right hand of a child who learns writing for the first time. The child's arm and hand have been used in larger activities, but the finer adjustments have not been practiced. When, therefore, the child and the adult begin the new task there is in both a general innervation of all the muscles and the larger movements are first made. The finer ones together with economy of energy appear later.

Writing in reversed slant appeared to a greater or less degree with all the subjects. In three cases the natural tendency was noticed in a few strokes and consciously continued; in two it was the result of letting the hand take its own position and "swing." Another tried to let the writing take its natural slant, which finally resulted in "back hand" script.

Improvement is characterized subjectively by a freeing of attention from the writing itself so that the observer is able to attend to details, to correct errors, and to make improvement in methods. Attention at first is so absorbed in the writing that the observer is not aware of his awkward methods. One by one he notices these and corrects them. Observers starting with good methods have fewer difficulties at the beginning and are able to anticipate them sooner. Attention is not only differently directed, but far narrower at the beginning than at the end of practice. As skill begins to develop, consciousness is wider and attention can shift from the task to extraneous matters with little disadvantage; where, as in the beginning, wandering of attention means distraction, and the work suffers. The relation, which exists between late and early conditions, exists also between the right and left-hand writing. In the latter, attention is easily disturbed; a strange pen, a slight illness, or a simple external hindrance have far more effect.

The general results of these tests with left-hand writing show the rôle of consciousness in learning of this kind to be corrective, its function being to criticise, to eliminate habits producing either physical discomfort or dissatisfaction with the product, and to make permanent any favorable variations which may chance to occur. The focus of consciousness

changes during the learning, attention at first being on the process itself, the details existing in consciousness only marginally or not at all. Later the learner attends to his methods and at the same time is more clearly aware of the elements leading to his satisfaction or dissatisfaction. As the methods are perfected they in turn become automatic, the learner assuming automatically the position which he has acquired consciously. As the process becomes still more automatic, attention wanders from it from time to time to foreign matters, without interference.

Mirror Script Experiments. Experiments in learning to write mirror script (that is, writing which begins at the right-hand side of the paper, and may be read by holding it up to a mirror or from the reversed side of the sheet) were carried on for a period of fourteen days with six observers, all of whom had served in the above mentioned left-hand experiments. The general conditions were the same as before. The subjects wrote the standard sentence three times with the right hand and three times with the left, and *vice versa* on alternate days, the writing of each sentence being timed as before except in two cases. No untimed tests were taken save in the case of two observers. As timing seemed to have no effect, untimed experiments were not made by the others.

The greatest difficulty was noticed by all observers in the first few trials, and consisted in knowing what the form of the letters should be. A certain amount of extraneous practice was allowed in order to meet this peculiar hindrance. Two observers began by writing on the blackboard with both hands at once, mirror script with the left hand and normal script with the right. This was easier than the writing with the pen, which required smaller movements. The other observers, seated at a desk with paper before them, were told to write the sentence in mirror script, after it had been explained to them what mirror script was, and were allowed to write the sentence, to hold the paper to the light and to correct mistakes.

Attention at the start was confined to the writing as a whole, but soon general difficulties decreased and particular ones were attended to, certain letter combinations being more difficult than others. After trying to make a letter of a certain more difficult form, the observers consciously chose a simpler style. As in the normal script experiments, excessive muscular tension was shown at first but later disappeared. With ease in writing, foreign ideas again begin to enter in every case; but attention cannot get too far from the process without disastrous results. One observer, for example, became so absorbed in a train of thought that he stopped writing. Ease of writing and freedom of attention, as before, allowed

difficulties to be anticipated and overcome before they were met. One observer consciously pronounced the difficult letters, because he found himself doing this in one instance with good results. Two observers visualized the movements, in difficult places, before making them. No observer mentioned attending to the process itself as a means to improvement, but two stated that attention to the process brought confusion.[1]

Had there been a good copy to give an idea of the letter forms, and had instruction been given as to position and relaxation of muscles in hand and arm, much of the difficulty would probably have been obviated.[2]

Learning to write, as evidenced in the above experiments, depends on consciousness mainly for perfection of methods. Adjustments which are at first "unconscious" become highly conscious then later automatic, a great degree of perfection requiring the third stage — automaticity. It is only as the grosser elements become automatic that attention is free to consider the finer ones.

A certain degree of difficulty is necessary to interest. As the task becomes automatic and easy, it is impossible to keep foreign ideas out of mind.

Progress is from coarse to finer muscular adjustments, and from larger to finer writing in most cases. This means a specialization of the smaller finger and hand movements, and a saving in energy, since less exertion is needed to call these into use, than for the larger arm movements.

Progress may take place without a high degree of consciousness, yet it will not go so far nor proceed so rapidly as when there is consciousness of the process itself.

5. *Learning to Multiply large Numbers Mentally*

The experiments of this series consisted in learning to square three-place numbers mentally, and were suggested by recent work of Thorndike in multiplying mentally a three-

[1]The matter of increased speed in the writing does not especially concern us here, though the records were kept and tabulated. It may be mentioned in passing, however, that there was very often to be observed an increase in speed in the left-hand writing, in the mirror script and even in the normal writing with the right hand, from the first to the third executions of the standard sentence in a single test—a transient gain in skill by practice of a particular set of movements.

[2]In any instruction it is just this which should be the function of the teacher, *i. e.*, to provide good methods and to call attention to errors which the narrow attention of the learner will not enable him to see. In a task like writing much mechanical repetition is needed; yet repetition without attention will not result in improvement but merely in the strengthening of abilities then possessed, and even of awkward procedures.

place number by a three-place number.[1] He found in the 33 observers who did from 28 to 96 examples of that sort, a gain of over 50 per cent. in skill, but gives no introspective results, except that strength of visual imagery was not responsible for the improvement, and that more individuals reported decrease than increase of visual imagery. My own experiments were carried out in a manner similar to his, except that for simplicity's sake my observers squared one number instead of multiplying two different numbers together. This made it necessary to hold but three digits in mind at the start, instead of six. In making the number list, digits above two were written on cards and drawn at random from a box. If a number contained the same two digits as the number before it, it was given a later place in the list, in order to avoid the distraction, or aid, of too great similarity.

The manner of conducting the experiment was as follows: A number was read to the subject, the observer repeated it, and the stop-watch was started. When he finished the example he gave the result, the watch was stopped and the time recorded. In the first day's trials the observer was asked to work aloud, but in some cases this proved a distraction and was not required later, though those who wished to work aloud were permitted to do so. In reckoning the results Thorndike's arbitrary method of transmuting errors into time by adding to the watch time one-tenth of its amount for each error made, was used.

Preliminary tests consisting in running through the forty-nine two-place numbers possible from combinations of digits above the digit three were carried out with two observers, and took six and five days respectively. In this short time great improvement was made; for observer A the average daily score in seconds for each multiplication was 51.1, 40.1, 35, 43.1, 41.9, 30.6, and for B, 81.7, 29.1, 26.2, 16.3, 21.4. (B's last score was raised by one exceptionally long time where the example was worked twice. If this one case be left out the last score is 16.2.)

Introspection showed that the gain was partly the result of the refreshing of the mathematical associations, *i. e.*, practice in multiplying and adding, but far more, of choosing and using new methods to avoid obvious difficulties, and of improvement of methods already in use. Ideas as to means of improvement were not the result of analysis previous to work, but came after some practical experience, when the observer was oriented and realized his deficiencies. The formulation of

[1]Thorndike: The Effect of Practice in the Case of a Purely Intellectual Function, *Am. Jour. Psy.*, XIX, 1908, 374-384.

such improved procedures was really a process of generalization. When the same peculiarity occurred several times, the observer recognized its universal character spontaneously, and not as the result of conscious search after it. The common element seemed to drop out of itself. Then, because of his strong desire for improvement, he consciously made use of it; but had he not been alert, it might easily have escaped his notice, and have been of no profit to him in his progress.

Instances of this process are the following: After a few examples *A* realized that, with the numbers in use, the answer must always have four digits. Visualization for this observer was at first impossible, the whole process having to be carried on in auditory-motor terms. The two partial products were retained as a sound whole; and to get the separate digits for the addition the observer must run through these products several times till the required digit was found. After several such experiences she realized that it was only in an auditory-motor way that work could be done, so in adding, she repeated the first partial product through as far as the units digit, held that in mind as the units digit of the complete product, repeated the first partial product again as far as the third digit from the left and the second partial product as far as its final digit, added these two and placed them in the tens place of the complete product, and so on. Later, visualization increased to some extent as the result of extreme effort, but remained almost entirely visualization of a special "form" into which the digits were fitted as they were required. After practice in addition, it was noticed that the first digit from the left of the second partial product and the last of the first partial product had no digits above or below them to add to them; and consciously less attention was given to them and more to the other four digits. Again, having worked slowly and deliberately so that one partial product escaped her by the time the other was obtained, she worked more rapidly in subsequent multiplications, spending more time repeating and emphasizing the results.

Observer *B* worked a single day, multiplying the numbers out by full multiplication. He then served as experimenter, with *A* as observer, and while so doing realized that the binomial method might be used, and used it in going over *A*'s work. In his next work as observer the change of method reduced his record from 81 to 29. The process was first as follows: Required to square 35. $a^2 + 2ab + b^2$, $35^2 = (30 + 5)^2 = 30^2 + 2.30.5 + 5^2$. Later he noticed that a^2 always ended in two zeros and simplified the process by simply setting together a^2 and b^2 then adding $2ab$. Then the method unconsciously came of getting $2ab$ while repeating $a^2 + b^2$, was recognized as a method and continued. Later, while making notes, it occurred to him that in multiples of numbers ending in 5, $2ab$ will be the first digit $\times$ 100. This was then consciously used with success. Superfluous words fall away in the process, only numerical results being given. This last was unconscious, however, as to intent. In all such work, a rule which is first conscious becomes an unconscious habit.

In squaring three-place numbers six persons served as observers, three men and three women, all of whom were university students. Observer V is the same as observer *A* of the two-place number experiments. The observers were practiced for two days on two-place numbers and three-place numbers, each subject working three three-place problems before the regular experiments began. The results included in the tables are only those of the regular experiments. The rec-

ords are based on the fifty examples worked by each subject, and cover between ten and fifteen days. Each worked half an hour a day. The method of timing was that described above for the two-place numbers. A rest of from two to five minutes between problems was given. If the original number was forgotten in the midst of the work, that problem was given up, and after a rest, a new one taken. As the observers worked at different rates, and as some forgot more numbers than others, the number of days taken varies. The following tables give the average results in errors, time and combined result for the six observers.

A decided gain is clear as far as speed is concerned, though accuracy seems to remain about the same, unless in the case of observer IV, where there is slight improvement. This, however, may be a matter of chance. The accuracy corresponds to one's skill with the addition and multiplication tables, which have been so much practiced that they have reached a "plateau stage" where no further improvement is likely. Improvement, in this experiment, is not in accuracy of work, not in the speed of computing (at least not to an observable extent) but in the ability to hold more things in mind and to attack the work directly and with more advantageous methods. The asterisks in the table indicate the points at which new methods were introduced. An asterisk occurring before the first day's score indicates that a method of work peculiar to the observer was developed in the preliminary three-place examples. Observer VI began the first day's regular experiments with a method which required the retention of but a few numbers at a time and made use of no new method. Observers I, II, IV had developed methods before this day, but made improvements in them during the progress of the experiment. It will be noticed that introduction of new methods resulted in a large drop in the time, except in the case of observer IV, who did not continue methods used after they were once developed, except the general method used on the first day.

The most difficult part of squaring three-place numbers by full multiplication was found, by all observers, to be the retention of the partial products long enough to add them, and to add the proper digits together. As they were allowed to work in any way they chose, the effort of each was to find some way to lessen this difficulty. Visualizers, as might be expected, had less difficulty than observers with little visual imagery. A brief account of the procedure of each of the six observers will best show the methods of improvement. Fatigue or distraction made retention difficult for all the observers and affected chiefly that part of the work.

TABLE III

Obs. I	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Errors	2.	.5	.6	.3	.25	.6	1.	.6	.5	1.6	1.2	1.	.6	1.0	1.0
Time	420.5	252.5	162.6	263.3	197.7	149.9	112.1	127.6	85.5	130.9	105.7	72.2	103.6	120.9	116.1
Comb.	504.1	265.7 *	173.4	269.	202.6 *	159.9	123.3	136.1	89.6	152.7	118.4	81.2	109.8	132.9	127.6

Obs. II	1	2	3	4	5	6	7	8	9	10	11	12
Errors	3.3	1.5	1.3	5.2	2.0	1.5	2.0	1.4	2.5	1.8	3.0	1.6
Time	102.7	140.3	113.5	98.3	71.6	78.7	86.9	87.1	104.7	81.4	60.8	52.3
Comb. *	136.5	165.8	128.6	174.9	85.9 *	90.5	91.3	99.3	130.8	96.0	76.5	60.6

Obs. III	1	2	3	4	5	6	7	8	9	10	11	12
Errors	3.	2.6	3.2	2.0	2.5	.6	1.8	1.6	2	1.6	1.4	1.2
Time	178.2	128.6	145.7	149.2	169.7	167.2	112.	134.4	145.8	124.3	127.8	108.
Comb. *	231.8	162.8 *	193.1	179.0 *	212.1	177.3	132.2	155.9 *	175	155.4 *	145.6	109.4

TABLE III (*Continued*)

Obs. IV	1	2	3	4	5	6	7	8	9	10	11	12	13	14	
Errors	1.3	1.3	1.0	1.3	.7	0	1½	1	.4	.6	1.2	.6	2.	3.	
Time	325.9	263.1	399.6	246.0	325.2	313.6	254.9	172.3	180.4	184.9	199.6	191.7	168.8	161.6	
Comb.	369.4	271.1	439.6	278.8	349.8	313.6*	263.4	187.3	187.6	196.0	227.1	204.5	202.6	210.0	

Obs. V	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Errors	2.0	2.0	1.3	1.0	2.	1.3	1.0	2.0	1.6	2.0	1.0	2.6	2.5	2.0
Time	332.9	409.3	304.5	333.5	285.1	222.6	206.8	157.7	219.0	152.6	240.0	242.9	151.1	177.7
Comb.*	399.5	487.9	345.1	366.8*	342.1	252.4*	227.5	189.0	163.8*	182.9	264 0	307.6	188.9	210.0

Obs. VI	1	2	3	4	5	6	7	8	9	10	11
Errors	4.0	1.5	1.6	2.0	1.1	1.5	1.0	1.0	1.0	1.2	1.3
Time	96.7	60.2	67.1	59.7	42.1	39.3	51.5	37.3	46.5	44.9	37.8
Comb.*	132.1	69.2	77.8	71.7	46.9	43.3	56.6	41.0	51.2	50.5	42.8

Observer I. This subject possessed no visual imagery for this sort of work. By the second example of the preliminary practice, he was consciously repeating the first partial product to the last digit, setting this in the answer; then the third digit of the first and the last of the second were obtained in the same manner and added, etc. As the required figures were taken out and embodied in the sum they were forgotten. The next day he tried to add two columns at once. On the day before the regular experiment he was permitted to make writing movements with his pencil, which he did henceforth, finding it an advantage. On the second day of the regular tests he tried remembering the numbers by pairs. (Notice the decrease on this day from 504.1 to 265.7.) On the fifth day the very advantageous idea of adding the first two partial products and adding the third to their sum was hit upon, thus having at no time more than two numbers to keep in mind. After this the drop is decided. The increase in time in the last three days was due to fatigue from other work earlier in the day.

Observer II. This observer is a good visualizer and has had experience in teaching mathematics. On the first day the observer tried to square two numbers, but did not succeed in either case, and refused to work. The next morning while thinking of other things, a method occurred to her, which she used with success throughout the work. It was to multiply the number by the multiple of a hundred which stood nearest, then by the hundreds digit multiplied by ten, and adding or subtracting the result, according as the first multiplier was below or above the original number, then multiplying by the units digit and adding or subtracting. Thus only two sets of numbers had to be retained and work was considerably in ciphers. For the first two days of the regular experiment a few seconds were taken to think out the method of work, but later this became unnecessary and the problem was attacked directly. The method had grown automatic. On the fifth day a permanent modification occurred in adding or subtracting by thousands, tens, and units visually. A question by the experimenter as to the way the work had been done suggested this.

Observer III—A visualizer. The first three-place number was declared to be "terrible," the greatest difficulty being to retain and add the partial products. In the second example she added the first two partial products and then the third. The next day the method was improved further, conciously, by getting the sum of the first two partial products before multiplying for the third. The regular experiments were begun with this method, which was afterward modified but slightly. The modifications were as follows: "putting" the first partial product on all the four fingers of the left hand and the second on all but the little finger and including the sum. This resulted in the thumb and little finger having only one digit, and the others two; this method was not continued. The third partial product was "put" spatially above the sum of the first two on the fourth day. On the eighth day she "put the first sum in the left ear" and got it again when needed. From the first, the observer automatically made writing movements on the table to accompany her work.

Observer IV (no visual imagery for this sort of thing). The observer developed no method until the sixth day except the repetition of the partial products until he came to the digit required for the addition, as described for observer I. He consciously hurried through the process of multiplication in order to spend the time on repetition and emphasis of the result. Notice the decided decrease in the score after the sixth day when the new method was taken by adding the figures as soon as they were obtained instead of first multiplying for each partial product. Subjectively the work was much easier after this way was taken.

Observer V (the writer, little visualization for this sort of work). The method of observer III, namely, the addition of the first two partial products was deliberately borrowed and used in the first day's work. The

calculation was consciously made as rapidly as possible that the parts might not be forgotten before the whole had been secured. On the fourth day the last two digits of the first sum were discarded leaving only a three-place number to add. Figures when they were required for addition were fitted into a visual "number-form" which had unconsciously developed. On the sixth and eighth days new methods were tried which worked well at the time, but were used only on that occasion. The first was to add the first and third products first, the second to place the digits on the fingers of each hand and set in proper juxtaposition. It was evidently forgotten. Two column addition was consciously tried and used in instances when the numbers to handle were not too large.

Observer VI. This subject developed his method in the practice tests and did not change it; improvement for him therefore consisted in practice in the use of his method. It was to break the number up into two numbers, the first consisting of the hundreds and tens and the last of the units, and to use the binomial method of squaring; in obtaining the square of the first (two-place) number, the a^2, the binomial formula was also used. This method occurred to the subject after he had gone to bed on the first day of squaring a three-place number by full multiplication, and he considered it an original method until several days later a distinct visual image of his old arithmetic book with its thumb-worn page bearing an illustration of a formula similar to this for extracting the square root, flashed up before him. Part of the original experience, without its localization in time or place, had been recalled, and given him what he considered an original idea. Stronger stimulation of the complex brought it back in all its original setting. By the third day introspection shows less attention and strain than at first, the method had become spontaneous and it was easier to keep two different sets of numbers in mind. Practice had resulted in a widening of the field of consciousness. The sixth day the observer said the work was easier because he got his results almost at a glance and when they came he saw the figures under the ones to which they should be added. When he added a certain column he saw only these figures distinctly; the others were hazy, but he could call them up when he wanted them.

At the close of the experiment, each observer was asked to give a report as to what he thought his improvement had consisted in, what part of the processes was conscious and what unconscious. All said the task was easier at the close than at first. Observer II, who developed a very simple method at first, said that after the first few days she was conscious of little improvement. Three observers attribute most of their improvement to the adoption of an easier method, and three assigned "practice" a large place. For all, the calculation itself was a highly conscious affair, though for three, the results sometimes seemed to come spontaneously. Adoption of new methods was in every case clearly conscious, and not from falling into a certain habit, noticing, and continuing it, as was the case in the target throwing and the writing. Very slight suggestions from one's own work or from outside were often responsible for the idea of the new way of working, or it "just popped in" as one observer stated. However it came, the idea had to be there to effect the change. After the method was practiced a little, it was used directly and with-

out thought. "Unconscious" improvement came in the widening of the conscious field, adaptation to the experiment and the like, so that the feeling of strangeness and awkwardness disappeared. What at first seemed an impossible task no longer looked so when one became oriented. Possibly another "unconscious" factor was the gain in speed in making computations as the result of greater familiarity with, or rather, refreshing of, the addition and multiplication tables. Mere practice caused improvement in the use of methods consciously adopted.

6. *Results of the Last Three Series of Experiments*

The results of the last three series of experiments seem to agree in showing that the function of consciousness in learning is to improve the process by bringing errors to light and correcting them, and by adopting improved methods suggested by some habit fallen into, or by some idea as to better possibilities. The more purely muscular the process to be learned, the less conscious the learning of it. In the target throwing improved methods of throwing came about of themselves and were not noticed until later. Attention to the mechanism only resulted in disaster. The most one could do consciously was to attend closely to the bull's-eye and throw, the proper co-ordinations seeming to take place of themselves; gross errors only were consciously corrected. In the writing experiments, consciousness played a greater rôle in supervising and correcting the process, and for some observers in starting an advantageous method. In the intellectual task of squaring a three-place number every decided step in advance was the result of a conscious change.

But these three grades of learning all showed "unconscious" improvement as the result of repetition (even the arithmetical computations), improvement which was entirely at the physiological level. Improvement, therefore, does take place without the control of consciousness. Yet even at the grade of learning where this is the truest, we cannot say that one is unconscious, but perhaps rather that marginal awareness, in the sense of organic and peripheral sensations, and feelings of satisfaction and dissatisfaction, is always present and affects the result. It is, one may conjecture, a feeling of dim awareness akin to this unanalyzable, undifferentiated state which accompanies the learning of animals low in the scale. That it *directs* the learning is at least not certain, if one reasons by analogy from human learning where only the higher, more specialized acts are under conscious control. The simpler and more "muscular" the learning, the more vague and

indefinite the subjective accompaniment. Practice alone is the improving factor. In more complex processes like writing the learner is able to assume an objective attitude and direct and criticise his own activities and to shorten, by choosing new methods or avoiding observable mistakes, a process which would otherwise require much mechanical repetition. In still higher operations, like arithmetical calculations, consciousness of the process is still clearer. It acts vicariously for practice, which takes a subordinate rôle. The rôle of consciousness is similar to that of the teacher who can do little for one learning feats of muscular skill save give a few simple instructions, leaving the rest for the pupil to get by the hit and miss of practice; but in more complex activities he can act as a pattern, giving methods and pointing out deviations from them. Since right methods and easier work result in a widening of consciousness, this will leave the pupil's attention free for still further advances.

Summary

In Part I we have considered consciousness as an ultimate fact, undefinable, identical with awareness. Unconsciousness denotes for us its opposite— entire absence of awareness, that which is entirely outside of our experience at any moment of time. Subconsciousness, for which we prefer to substitute "perceptual" factors, gives focal consciousness its qualitative character. Subconsciousness is *consciousness* of a less distinct degree. Divided consciousness, such as is present in cases of multiple personality, is best called "co-consciousness."

The question of the existence of "unconscious psychic processes," *i. e.*, psychic accompaniments of physiological processes lacking awareness, depends for its answer on one's metaphysical concepts, which are in the end a purely temperamental matter. Denying them leads to the interactionist position. Throughgoing psychophysical parallelism demands the assumption of psychic factors accompanying physiological changes, and this position we have taken, insisting, however, that such "psychic" processes are qualitatively different from anything which enters into consciousness. According to such a view one may speak of physiological processes in addition to "unconscious psychic processes."

We have incidentally reviewed the arguments *pro* and *con* as to the presence of "unconscious" factors and their influence on mental phenomena. Our main interest was, however, in the relation of consciousness to learning. From general observation we have seen no case of learning where one is absolutely unconscious, yet one may be unconscious of the

end, the process and even of the development of the habit or association itself.

Our own experimental results are the following:

1. Our experiments on the nonsense-syllable material give chiefly negative results, but justify, so far as the conditions of the experiment permit, an inference that what is entirely outside of consciousness, though it is in such a position that it might easily become conscious, has no great effect, positive or negative, on the learning of the same material when it is presented later to clear attentive consciousness.

2. In the experiments on the "Motor Set" (*Motorische Einstellung*) we find that a habit may be formed despite the fact that one is unconscious that one is forming it. Yet, withal, attention to the task produces in all cases a more definite habit, a stronger "*Einstellung*," than that which is caused when one is almost unconscious of his performance. Attentive consciousness without doubt is accompanied by greater tension in the particular muscles involved in the current activity of the organism and in their nervous connections. Here activity is concentrated. The more fully the physiological mechanism is thus put into activity the more it is affected in the direction of easier and more efficient activity of the same sort.

3. The experiments on throwing at a target involved learning of a sensory-motor kind, the doing of a definite thing: it was practice with a fixed aim in view. Here focal consciousness was almost entirely projected on the target, the ball and hand occupying a peripheral place. Conscious control was exercised only over the grosser parts of the process. Methods gradually changed, and improvement appeared, without conscious change or control. The sensations from the arm and body no doubt contributed to the improvement, but these were always at the "perceptual" level and consisted rather of an undifferentiated background.

4. In the writing experiments conscious direction of the process and methods was more marked. At first consciousness is bound down to the general execution of the task. The more general, larger elements, becoming automatic, leave consciousness free to turn to details, when disadvantageous methods are noticed one by one and eliminated. Unconsciously modifications in the method crop out, and as consciousness becomes freed from details these are noticed, practiced, and improved upon. This sometimes results in a considerable change of adjustment of the different factors.

5. In the experiments on mental multiplication consciousness had a more immediate effect than in the more "muscular" sorts of learning. Here advantageous methods occurred to the

subjects while they were working, or between the experiments, and when these were adopted the improvement was immediate and permanent, whereas in the more "muscular" sorts of learning one's muscular co-ordinations had to be practiced somewhat before the new method was perfected. In the number experiments, just because one is alive to the situation, he notices clumsy methods and slight errors, and is therefore ready to improve upon them. After a method was consciously developed, however, it was soon used unreflectingly—it became a habit. In proportion as an activity is conscious, consciousness is an aid or even an essential factor in its acquisition. This applies to details and part-processes as well as to the larger units of activities.

Conclusions

In conclusion we may say that in learning of any sort both conscious and unconscious factors exist. Unconscious factors are those involved in the fixing of the association by practice, and the cropping out of modifications of behavior subsequently utilized by consciousness.

The more intellectual and highly conscious the material to be learned, the more direct and immediate the effect of conscious control. Practice results in a standing out of common features of the process; these are focalized, and generalized into rules for new and better procedure, which immediately takes place.

In complex processes involving both an intellectual and a muscular side, the activity as a *whole* is conscious. Details are gradually mechanized, leaving attention free to attack new difficulties. Factors of the activity which are at first only at the "perceptual" level become clearly conscious, are then practiced and improved upon, and finally become mechanized and unconscious again. Consciousness is a corrective agent, eliminating errors, improving on elements unconsciously developed, and organizing the whole procedure.

In learning simple muscular co-ordinations consciousness is focussed entirely on the end—on the outcome of the movement. One is only dimly aware of the different sensations and feelings entering into his bodily adjustment, and should any of these become the object of attention, disturbance of co-ordination results.

Learning can progress, however, without consciousness of the end or of the fact that one is learning, but even here a high degree of attention to one's task brings more marked results than work under distraction.

So far as our experiments go, factors never entering consciousness have neither a beneficial nor hindering effect on the learning.[1]

[1]The writer wishes to express his obligation for the faithful service of those who served as observers in the above experiments, and particularly to Dr. E. C. Sanford, in whose laboratory the work was done and at whose suggestion the subject was begun.

Appendix

Experiments with Meaningless Syllables

The general plan of these experiments has already been described in the body of the paper (pp 179 ff). It is only necessary here to record the details of procedure. The work was done with twelve-syllable series of meaningless syllables prepared in accordance with the method of Müller and Schumann's "*verschärft normal*" series,[1] except that additional letters were used to increase the possible number of syllables and to adapt them to English speaking observers familiar with German. There were 20 initial consonants and double consonants, (b, d, f, g, h, j, k, l, m, n, p, r, s, t, v, w, z, th, sh, ch), 19 finals, (b, d, f, g, j, k, l, m, v, p, r, s, t, v, x, z, th, sh, ch) and 14 vowels and diphthongs (a, e, i, o, u, y, ä, ö, ü, ai, oi, ee, oo, ou).[2] These syllables were presented by means of a rotating drum of the intermittent-movement type, manufactured by Spindler and Hoyer of Göttingen, which permitted the syllables to remain at rest during the greater part of the time of their exposure. (See Fig. II which shows the apparatus from the back and side.) The syllables were seen through the opening of a suitable screen (See Fig. III) in such fashion that a single syllable of the series to be learned appeared each time between two syllables of series with which at the moment the observer had nothing to do. Thus the syllables

tam pog bex

would be shown, the observer being required to learn the middle series to which *pog* belongs, but having nothing to do at the time with the series to which *tam* and *bex* belong.

The observer sat before the screen and read the syllables of the middle series as they appeared through the slit. The experimenter sat at the side of the machine to the observer's right, his movements being entirely concealed from the latter by a large screen of gray cardboard. Directly in front of the apparatus and resting on the same table was a second drum, a portion of the surface of which could be seen through a slit in the black cardboard screen before it. (See Fig. III.) The syllables were all learned by the "Treffer method," and the "Treffer syllables" were shown on this drum, which was turned by the subject as the syllables were required.

The syllables to be learned were written on strips of white paper 12½ x 3 inches, ruled with fourteen lines and so proportioned to the drum, that after one complete presentation of the series, two blank spaces were shown before the first syllable of the series reappeared. The odd numbered members of the series—the "Treffer syllables"—were also written on strips 10½ x 3 inches to fit the smaller drum.

The experiments fell into three series, *A*, *B*, and *C*, and were carried out with two trained observers *S* and *E*.

[1] Müller u. Schumann: Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zeit. f. Psych.*, VI, 1893-94, 106.

[2] The series were prepared throughout the greater part of the work by a competent assistant not otherwise connected with the experiment.

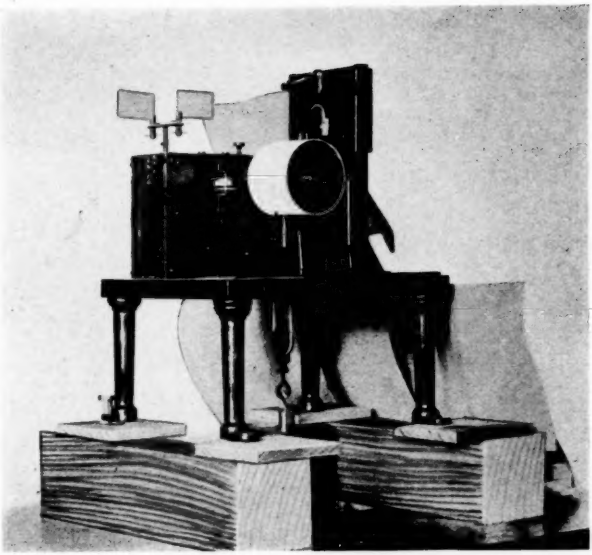

Fig. II

Experiment A. Problem: If a series of syllables is presented within the range of clear vision alongside of a second series which is to be learned, and engages the observer's attention, to find whether such a side series to which no especial attention has been given will later be more easily learned because of its exposure to possible "unconscious" perception. If Sidis's and Scripture's experiments are valid in their results, it was judged that we should find evidence of "subconscious" learning in the more ready memorizing of series which have been so exposed.

Three of the above mentioned twelve-syllable series were written in three vertical columns, so that the first syllable of each series appeared to the observer through the slit at the same time, then, as the drum turned, the second syllable of the three series together, and so on to the end. These series we shall call *a, b, c,* reading from left to right, *b* being the middle series, and the one learned.[1] On a second strip were written three series, *d, a, e; d* and *e* being entirely new series and *a,* the one which had stood to the left of *b,* the first series learned. On a third strip was written an entirely new set of three series, *f, g, h;* on a fourth strip, *i, h, j,* of which *h* had stood to the right of *g,* the series previously learned. All ten of these series were *"verschärft"* series, and the ten together constituted a regular set.

The number of series which could be used for each subject was, from the nature of the case, limited (11 sets for *S* and 12 for *E*). The method of obtaining syllables enough for all the experiments and avoiding familiarity with the syllables as much as possible was this. Syllable series which had been side syllables for *E,* but not learned (there were 6 such in every set), were transposed so that they occupied the places of *b, a, g* and *h* which had been learned, and were then given to *S,* and *vice versa.* Thus, if series retained their former lettering they would read *j, i, h; g, j, f; e, d, c; b, c, a.* After each subject had learned these transformed series, *E.* was given the transformed series for *S,* and *S* for *E.* Then each was given the series the other had learned first, but the syllables of the individual series were shifted. This made a possibility of 552 new syllables for each subject. When these had been run through, the second and third permutation (which the subject had learned before) was repeated.

An extra series (indicated in what follows by *x*) was also used each day and was obtained by taking a strip from an unused set and learning the middle series. One set thus divided furnished extra series for four days. This extra series was given sometimes first, sometimes third and sometimes fifth in the order of the day, each subject being kept in ignorance of its position, so that he was never aware of which series he was learning. The order which was maintained for the learning was, therefore, either: *x, b, a, g, h;* or *b, a, x, g, h;* or *b, a, g, h, x.*

The odd numbered syllables of the middle series of each strip were written in the centre of a slip fitting the "Treffer" drum; here, therefore, there appeared through the slit only one syllable at a time instead of three. For the first three days, the "Treffer" order was given according to the permutations of Müller and Pilzecker[2] but afterwards the "Treffer" syllables were written in a direct order from one to eleven, as it was decided that a varied scheme would give less uniform results with a moderate number of experiments, and would not in any case influence the point at issue in the experiment.

The average time taken for ten revolutions of the drum during the practice work and for two days of regular work was 87.1 seconds for ten revolutions or 0.62 + seconds per syllable. This speed was accelerated on March 10 (the third day), and from then on was 80.2 sec. per 10 revolutions or 0.57 + second per single syllable.

[1] The middle series is always the one learned in these experiments.

[2] Müller u. Pilzecker: Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zeit. f. Psych.* Erg. Bd. I, 1900.

Each observer served as experimenter when the other was observer. Both knew the object of the experiment, but neither knew the results until the experiment was finished, as no accounts were cast until the end. With the exception of the first two days of regular experimentation, when *E.* worked at 2 P. M., *E.* served as subject from 8 until 9 A. M. and *S.* from 12 to 1 P. M. (The time for *E.* was changed because syllables which *S.* had recited at 12 persisted and acted as a disturbing factor.)

From January 24, 1909, to March 7 practice work was carried on every day except Sunday and four days of the Easter Vacation. Regular work began March 8 and was carried on every day except Sunday and one day when *S.* was out of town and another day for *E.* when the machine was out of order, making 25 days of regular work for *S.* and 24 for *E.*

At the words "In your place" the observer took his seat before the machine. This was started and the drum allowed to make one revolution to get its speed; as the first blank space appeared the experimenter called "ready," and lowered a shutter previously hiding the drum. The observer began to read as soon as the first syllable appeared, reading through the whole series of twelve syllables. After the two blank spaces had passed the series was read through again, and so on, until it had been repeated twelve times. The syllables were read pair-wise, at first in trochaic rhythm; later both observers fell into the iambic. When the last syllable was read on the twelfth revolution, the experimenter raised the shutter and started a stop watch. The observer began immediately to turn the "Treffer" drum, which was adjusted so that a single blank space preceded the first "Treffer" syllable, and read each "Treffer" syllable as it came up, giving, if possible, its associated syllable (*i. e.*, the syllable which had formed the other half of the pair); when he could not recall a syllable he said "don't know" and passed on to the next. When the last associated syllable had been given (or given up) the stop watch was stopped. This made a somewhat rough method of timing, but was effective enough for this experiment in which time was only a minor consideration. The observer was neither hindered nor helped by the fact that he was being timed. A list of each series to be learned had previously been written in a blank book and opposite each syllable was placed a check mark, if the correct syllable were given, a dash if none came, or the syllable which was given if a wrong one was given. After testing his associations with the "Treffer" syllables the observer's introspections were taken on such items as the difficulty of the series, the conditions of his attention, and influences which might have favored or been disadvantageous to the learning or reproducing of the series. The whole process—repeating the series, giving the associated syllables and the introspections—took, on an average, two and a half minutes. Ten minutes after the first series was begun, the experimenter again called, "In your place," and began the second series by starting the drum and, at a ready signal, letting down the shutter. Between the learnings a free time of about seven and a half minutes elapsed. In this interval the observer was allowed to relax as he pleased, either in looking over books, walking around the room, or gazing out of the window. But any taxing occupation which absorbed the attention to a considerable degree was avoided as it was found to have an unfavorable effect on succeeding series.

In computing the results a unit was allowed for each perfect syllable, 6 therefore being the score for a perfect record, *i. e.*, the recall of each of the six even numbered syllables. An average was taken for each of the series for the whole period of the experiment (24-25 days). A modified average was also made in order to include partial successes as follows: One-third was given for each vowel, diphthong, consonant or double consonant correctly given. Thus a syllable having only the vowel correct would score $\frac{1}{3}$ and one having its two consonants or a consonant and the vowels, $\frac{2}{3}$.

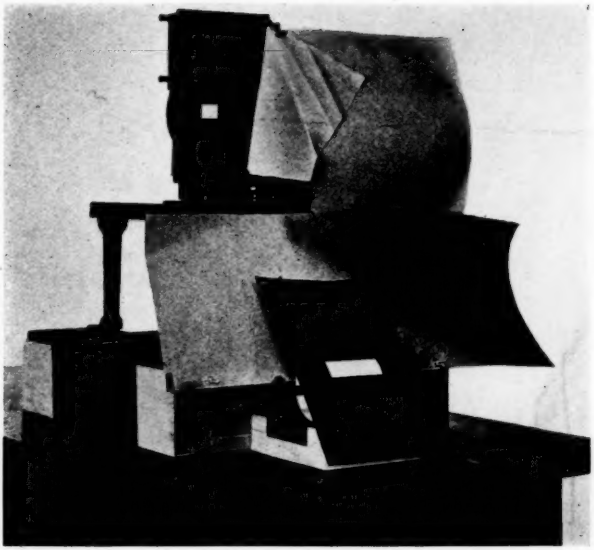

Fig. III.

Fig. III. In this figure the screen of the upper drum is arranged to show but a single syllable, an arrangement used in Experiments *B* and *C* below. In Experiment *A* it was open full width horizontally and showed three syllables at a time. The treffer drum is seen below behind the black cardboard screen.

The average of these results gives the modified average. Stated in tabular form the results are as follows: The figures in parentheses stand for the averages reckoned from perfect syllables only, and those standing free, for the modified averages.

TABLE I

Series	b	(P. E.)	a	(P. E.)	g	P. E.	h	P. E.	x	P. E.
Obs. S	(2.62)	± .21	(3.03)	± .19	(2.86)	± .12	(2.86)	± .12	(2.91)	± .18
	3.12		3.21		3.24		3.19		3.34	
E	(3.28)	± .13	(3.09)	± .17	(2.42)	± .16	(2.86)	± .19	(2.84)	± .15
	3.55		3.33		3.00		3.00		3.28	

For observer *S* the *a* series has a slight advantage over the *b* series, but the *P. E.* is so large that its advantage is quite uncertain. The *h* series is inferior to the *g* series. Taking account of subjective conditions we find that the *a* series was on the average learned under more favorable circum-

stances than the *b* series, and the *g* series than the *h* series; *i. e.*, there were more "mnemonics," the syllables were easier, or attentive conditions were better. For *E* the *a* series is poorer on the average than the *b* series, but the *h* series is better than the *g* series, if only perfect syllables are considered; if the modified averages are compared the two are equal. Here subjective conditions are slightly in favor of the *g* series. We may therefore infer from the results of both observers that the mere fact of having been shown as a side series does not favor that series when that series itself is to be learned—at least not to a degree sufficient to be detected by this method of experimentation.

If we arrange the averages of each series according to the position it occupied in the day's programme we have the following table:

TABLE II

Place in day's programme	Observer *S* 1st	2nd	3rd	4th	5th	Observer *E* 1st	2nd	3rd	4th	5th
X	3.2		3.		2.5	3.0		2.9		2.8
B	2.8	2.3				3.37	3.			
A		2.9	3.25				3.19	2.6		
G			3.3	2.68				2.43	2.42	
H				3.3	2.75				3.29	2.77
Average	3.0	2.6	3.18	2.99	2.62	3.18	3.09	2.64	2.85	2.80

The figures on the first line, reading across, denote the place in the day's work; the first column gives the name of the series. The partial successes are included in the figures used for this table. These figures show that when series of a certain denomination come early, they almost without exception show better results than when they occupy a later place in the experiment, and there is also a tendency to a general decrease from the first to fifth place as the X series and the averages show. We find a slight exception for *S*, where there is a rise for the third series of the set after a low score for the second series. Subjective conditions probably account for the increase here. What we had therefore in Table I is probably only the result of this general tendency.

Experiment B was next undertaken. The problem was to find whether a side series actually read a single time with full attention would be learned more readily for that fact, if between the reading and the learning, another series were learned. The apparatus and conditions of the experiment were those for experiment *A* with the following changes: Two pieces of black cardboard were made to fit in a groove under the slit in the screen. One was a straight piece which was just long enough to cover the middle and one side syllable, letting the other side syllable show. This could be slipped to the right or the left, exposing the syllables of whichever side was desired. A second piece of cardboard had a square hole cut in the middle, so that when it was slipped into the groove, only the middle one

of the three series appeared. One or the other of these pieces of cardboard was kept before the slit all the time, so that only one syllable ever appeared to the subject at a time, and that a syllable of the series being read or "learned." The cardboards were manipulated by the observer. The procedure was as follows: One series was read a single time by the observer, the shutter was closed by the experimenter and the cardboard changed or shifted so that the series to be "learned" would show; the experimenter lowered the shutter at the end of the one intervening revolution, and the observer then began the reading of the series to be "learned." As before, ten minutes intervened for rest and introspections between the beginning of successive series. The programme in outline is as follows,—

Read *a* once, and wait 1 revolution of the drum
"Learn" *b*, i. e., read *b* 12 times
" *a* " " *a* " " beginning 10 min. after beginning *b*
" *g* " " *g* " " " " " " " " *a*
" *h* " " *h* " " " " " " " " *g*
Read *i* beginning 10 min. after learning *h*; wait 1 turn of the drum
"Learn" *j*, i. e., read *j* 12 times
" *i*, " " *i* " " beginning 10 min. after beginning *j*.

The *a* series was always written at the left of one strip, the *b* series in the middle, the *g* series in the middle and *h* on the right of a second strip, the *i* and *j* series on the left and right, respectively, of a third strip of paper. No extra series were used. The *g* and *h* series, which were learned without having been previously read were used as a check on the results of the other four series.

The experiment extended from April 14 through April 24, omitting the intervening Sunday, making ten days of experimental work. Twelve repetitions were used for each series except for *S*, for whom the number of repetitions was reduced to ten on the seventh day, because he was frequently getting more than half the syllables right.

The results are given in the following table:

TABLE III

Series	b	(P.E.)	a	(P.E.)	g	(P.E.)	h	(P.E.)	j	(P.E.)	i	(P.E.)
Obs. S	4.7	.36	4.6	.19	4.5	.26	3.7	.26	3.7	.26	4.7	.24
	4.7	.36	4.3	.27	4.1	.15	3.5	.23	3.3	.23	4.5	.31
E	3.7	.15	3.2	.24	3.3	.30	3.4	.17	3.2	.28	3.4	.32
	3.5	.23	3.0	.23	3.0	.17	3.1	.21	2.8	.28	3.3	.37

The second line of averages, reading across, are those for perfect syllables only.

If a single reading of the series before learning a second series has helped the first series when it was learned ten minutes later, the *a* series will be better than the *b* and the *i* than the *j*. But considering the large P. E. neither series for *E* is helped by its reading, nor the *a* series for *S*. But for the latter the *i* series shows a marked superiority to the *j* series. This is in part explained by the introspective accounts which show that this series was favored by slightly better conditions, as ease of syllables and attention paid while learning the series. The superiority of the *g* over the *h* series is also to be explained in the same way.

It was thought, after obtaining these results that perhaps the *a* series did not show an increased average because the average of the *b* series was kept high by the fact that it was the first series and, for that reason, learned with special energy; also, the *i* series might have been favored above the *j* series because of a renewed impulse to succeed which often comes when one is almost at the end of his task. Therefore, a third variation was tried as follows:

Experiment C. The two indifferent series, *g* and *h*, were made respectively the first and last series learned. The places of the *a* and *b* series on the drum were interchanged, and also those of the *i* and *j* series to avoid any effects which position at the right or left of the paper might have had. The principle of the experiment is, however, precisely that of Experiment B, viz., one series was read, a second learned, and then the series which had been read was learned. The time intervals are those of Experiment B. The scheme in outline is:

Learn *g*	Read *j*
Read *b*	Learn *i*
Learn *a*	Learn *j*
Learn *b*	Learn *h*

In order to get as exact information as possible as to the relative ease of the series compared, account was taken of all the "mnemonics" and of slight associative aids. After giving the syllables associated with the "Treffer" syllables, the observer was shown the series again and asked to give with that help any "mnemonics" or other aid he had had in learning the series, and these the experimenter noted down.

The experiment extended from April 28 through May 10, omitting Sunday, May 2, and covered ten experimental days. The results are given in Table IV. The second line of averages for each subject takes account of perfect syllables only.

TABLE IV

Series	g	(P.E.)	a	(P.E.)	b	(P.E.)	i	(P.E.)	j	(P.E.)	h	(P.E.)
Obs. S	4.7	.17	4.5	.22	3.9	.23	3.4	.30	4.2	.33	3.7	.20
	4.2	.14	4.2	.23	3.6	.25	3.1	.32	3.7	.26	3.5	.25
E	4.2	.21	4.3	.24	4.1	.30	2.9	.40	4.0	.24	3.7	.31
	4.1	.21	4.1	.26	3.9	.32	2.6	.42	3.7	.23	3.5	.31

If reading the series were a help, the *b* series would show better results than the *a* series, and the *j* than the *i*, but this is not the case except for the *i* and *j* series when learned by *E*, where, despite the large P. E., the *j* series shows a real superiority. This is due to the fact that on two of the experimental days the *j* series was extraordinarily better than the *i*, and if these two days are left out of consideration the difference is too slight to be of importance. As experiments *B* and *C* are really two divisions of the same experiment, there being no difference in character, we may average the results of the two, which will give the results for twenty days experimentation. Series 1 and 3 are those learned without previous reading, and 2 and 4 those learned with one reading before an intervening series was learned. They are contained in the table following; only the averages including partially correct syllables are given.

TABLE V

Series	1 (P. E.)	2 (P. E.)	3 (P. E.)	4 (P. E.)
Obs. S Av.				
Exper. *B*	4.7 .36	4.6 .19	3.7 .26	4.7 .24
" *C*	4.5 .22	3.9 .23	3.4 .30	4.2 .33
Av.	4.6 .29	4.3 .21	3.6 .28	4.5 .29
Observer E				
Exper. *B*	3.7 .15	3.2 .24	3.2 .17	3.4 .32
Exper. *C*	4.3 .24	4.1 .30	2.9 .40	4.0 .24
Av.	4.0 .20	3.7 .27	3.1 .29	3.7 .28

If the extra reading was of advantage, Series 2 should be better than series 1, and 4 than 3. This is indeed the case with series 4 for *S* and to a slighter degree for *E*, but for series 1 and 2 the required relation is exactly reversed. Unless one is willing to infer that the preliminary reading is a hindrance at the beginning of the day's session and a help at its close (for which there seems no obvious reason), one is forced to regard the result of the experiment as negative. Subjective conditions, such as better attention, will perhaps explain the difference.

In conclusion we may say, then, that the results of all our experiments with meaningless syllables were negative. The presence of a series on the side of a series learned did not cause this side series to show more facile learning than the series not so aided, at least to a degree discoverable by our method. Nor did actually reading the series before learning a series give clearly better results for this series when it was learned later.

CONSCIOUS ATTITUDES[1]

By HELEN MAUD CLARKE

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The term *Bewusstseinslage*, in the sense of 'conscious attitude,' was introduced into experimental psychology, at Marbe's suggestion (1901), by Mayer and Orth, who employed it to characterise certain conscious phenomena, describable neither as determinate ideas nor as volitions, which appeared in the course of a qualitative study of association. These phenomena are referred to by Marbe himself (1901) as "obvious facts of consciousness, whose contents, nevertheless, either do not permit at all of a detailed characterisation, or at any rate are difficult to characterise;" instances are doubt, difficulty, effort, assent, conviction. Marbe offers, then, no definition of the conscious attitude; he gives only a negative criterion and a list of examples. Messer (1906) adds to the list, and at the same time extends the range of the term, using it to include experiences of logical relation, of the meaning of words and sentences, etc. Bühler (1907) restricts the attitudes to the "mehr zuständlichen Erlebnisstrecken, die als Zweifeln, Besinnen, Abwarten, Erstaunen, usw. beschrieben werden." Marbe seems not to approve of the restriction: "die neuerdings versuchte Einschränkung des Begriffes der Bewusstseinslage entspricht nicht den Ausführungen Marbes," declares

[1] From the Psychological Laboratory of Cornell University.

a recent writer from the Würzburg laboratory; and the implication is that Marbe holds to his original ideas.[1]

It is clear that to attempt a definition of 'conscious attitude' at the present time, would be premature. We use the phrase to denote certain large and comprehensive experiences, not evidently imaginal in character; and it is our aim, in this study, to bring these experiences to the test of introspective observation, and thus to discover whether or not they are analysable. Whatever be the outcome of the discussion of 'imageless thought,' it is probable that the name 'conscious attitude' will be retained as a descriptive term; but how the attitudes are to be distinguished, analytically, from 'thoughts' on the one hand, and 'emotions' on the other, must be left for the future to decide.

Incidental Analyses

The conscious attitude is, in general, an elusive experience, which it is difficult to secure in isolation; we therefore resorted in the main to an indirect method, and sought to arouse conscious situations in which various attitudes would be likely to occur. The observers were instructed to give complete introspections. The stimuli in the first series of experiments were letters or words which were written in the blind point-alphabet, and were perceived tactually. At the first sitting the observer was given a slip bearing several letters in their alphabetical order, and was allowed to feel them, and to associate the name to the tactual perception in any way that he chose. Then letters or words were given him to be recognised. The experiments by this method fall into two periods. Those performed during the spring term of 1909 were tentative, and their results were used for the improvement of the method. The stimuli for single experiments were short words or nonsense-combinations of letters. The introspections were written by the observers.

When the work was taken up again in October, the method was somewhat modified. Only single letters, the first ten of the alphabet, were used as stimuli. The whole of a word, perceived tactually, cannot be 'taken in at a glance' as in visual perception. If the letters were mere nonsense-combinations, each letter had to be recognised separately; the experiences thus became complex, and the introspective report might be incomplete. If the letters made a word, the observer tended to interpret from context, which further complicated the problem. In the new series, the reports were dictated, and writ-

[1] On conscious attitudes in general, see E. B. Titchener, *Exp. Psychol. of the Thought-processes*, 1909, 98 ff., 270, etc.; E. von Aster, *Zeits. f. Psychol.*, xlix., 1908, 60 ff.; on Marbe's position, M. Beer, *ibid.*, lvi., 1910, 265.

ten down by the experimenter. The most important change in method, however, was the measurement of the reaction-time. The instrument used was a Vernier chronoscope, one key of which had been replaced by a lever arranged just above the letter to be felt. When the observer touched the letter he moved the lever, and thus broke a current and released the pendulum. A finger of the left hand rested throughout on the other reaction key; as soon as the observer recognised the letter, he pressed this key, and released the other pendulum. In these, as in the earlier experiments, the observer was given the letters, in alphabetical order, to feel at the beginning of every hour, unless he declared, of his own accord, that the letters were clear in memory. There was constant instruction to reduce all experience to its lowest terms.

The observers were Dr. Pyle (P), Dr. Okabe (O), Mr. Foster (F), and Miss de Vries (V). The first was at the time assistant in psychology, and the other three were graduate students of considerable experience in introspection. When the experiments were given in their changed form, Dr. Geissler (G), instructor in psychology, took the place of Dr. Pyle. Miss Mary Clarke (MC) and the writer (HC) were observers in some later experiments. MC was untrained in psychology, though advanced in other lines.

1. *Types of Observers*

P is predominantly verbal in type. He reports verbal ideas in sentence form, with a few visual images, sensations of strain, organic sensations and feelings. Thus, CAB. cab. "I perceived the first letter and said C, but had verbal ideas like 'I am not sure whether it is or not.'"

V represents a mixed type. Visual images play a large part in her consciousness, and many of these are colored. Recognition is often mediated by a tactual image on the finger. Verbal ideas, affective processes, kinæsthetic and organic sensations are also numerous. Thus, DC. dc. "Felt the dots and said d, c. Unpleasant. Got visual image at once from the feel, and then said the letter." E. e, 1. 20. "Very pleasant. Sensations from mouth in smiling. The dot fitted into a hole in my finger which corresponded to E. Reaction automatic and hardly conscious. I visualised the round dot and a printed E. Smiled at the similarity of these."

O is also of a mixed type, reporting visual, verbal and auditory images in great numbers. Organic sensations and strain play a great part in his consciousness, as do also affective processes, kinæsthetic and tactual images, pain and temperature sensations. Thus, I. i, 5.12. "Attention well concentra-

ted and body well adjusted. When I touched the lever, shock from muscular contraction. Vacant consciousness, state of waiting. Organic sensation like irritation in back. Stamping of foot, frowning, extreme unpleasantness. Feeling of motion on left finger; probably a real motion, not an image. Suddenly a visual-verbal image I."

F reports verbal, visual and auditory images; more auditory than any other observer except O. Kinæsthetic images and organic and strain sensations are also reported, as well as a few affective processes. Thus, IF. if. "The two letters have different organic sensations. I cannot describe them except by saying that that which is with I is long, and that which is with F is broad; both are narrow, like the letters themselves."

In the introspections of G, all imagery except verbal is almost entirely lacking. Sensations, both kinæsthetic and organic, and affective processes are prominent in consciousness. When G felt the alphabet in order to learn the letters, he described every letter aloud, and associated the verbal description with the tactual perception. G reports that his consciousness in general is almost without images, and his memory of scenes and events is in verbal form. He places things spatially by means of eye-movements, which are unaccompanied by images. Thus, B. b, 4.42. "At the first touch perception of a long row of dots with absolute clearness. It confused me, that is, it excited a complex of organic sensations, and the verbal idea, Why can't I tell what it is? Organic sensations especially from diaphragm. I said, I have to see whether there is a dot below or a changed dot at the end. Then another careful touch of the letter, with the little dot below most prominent. Verbal ideas: Oh, yes, that is B. The whole strongly pleasant."

MC is of a pronounced visual type, but reports some imagery from other senses.[1] Thus, *Symposium.* "Verbal image of the word with sensations in throat. Visual image of a picture of a Greek symposium from a Greek history, followed by image of a table with people seated around it. Image of a convention and auditory image, Round table."

HC is of the same type of imagery as MC. Thus, *Reliability.* "Visual and verbal images of the word. Very vague visual image of some kind of support bearing a heavy weight."

2. *Attitudes Analysed*

In going over the introspections, to discover what attitudes have been analysed by the observers, we meet with some am-

[1]For the method employed with MC and HC, see p. 236 below.

biguity and confusion of terms. So far as attitudes have been named, they have been listed under the name applied by the observer. With this word of caution we proceed to consider the analyses of various attitudes to be found in the reports.

APPROVAL. V. Pleasantness, with some general kinæsthesis.

AWFULNESS. G. Once analysed as a strong unpleasantness and frowning, and again as the same with the addition of inhibition of breathing.

BAFFLED EXPECTATION. O. Visual image of face and frowning forehead. Left foot stamped. Slight burning sensation in back. Bodily and mental attitude was adjusted to a more difficult letter. Partial relaxation and muscular strain in upper part of body. Organic sensation and muscular strain which I could not localise.

CAUTION. V. Verbal idea, Be careful.

COMFORT. V. Organic sensation and smiling.

COMPARISON. HC. The two things were side by side, visually.

CONFIDENCE. O. A good adjustment of muscles, and sensations from them. Agreeable organic sensations. V. "I sat up straight and took deep breaths, and had a sense of stiffness in the spine, and pleasantness."

CONFUSION is analysed by G as "a complex of organic sensations, and verbal idea, Why can't I tell what it is?" "Unpleasantness, organic attitude of inhibited movement of diaphragm and of breathing." F analyses it once as a number of conflicting strains.

O reports the repeated appearance of conflicting verbal ideas: "It may mean B and it may mean C," unpleasantness, and organic sensations in abdomen which seemed to travel upward and to be checked by some obstacle. "Several initial associations of auditory-kinæsthetic images. Muscular strain in head, organic sensation in trunk." V reports holding of breath and blank consciousness.

CONSCIOUSNESS OF FITNESS. O. Relaxation of organic and muscular strain, stretching of back, long breathing.

CONSCIOUSNESS THAT THE LETTER WAS TOO SMALL. O. Muscular strain and organic sensations.

DECISION. O. Slightly agreeable feeling and successive auditory images, It must be so.—Auditory image. Kinæsthetic bodily attitude. Oganic sensations.—Bodily attitude and relaxation of muscular strain.—Memory image, visual or tactual or both, of E. Change of organic and muscular sensations. (This attitude is analysed several times by the same observer in the same way.)

DECISION TO DISREGARD THE LEVER. F. Visual and kinæsthetic images of this action. I saw myself doing it.

DETERMINATION. G. Pleasant mood. Verbal idea, I am going to do that well. F. Nodding of head toward finger, and attending to tactual image of coming sensation. V. Verbal ideas, biting the teeth and jerking the head.

DIFFICULTY. G. Trouble to get a verbal idea. Three ideas a, e, and i were ready, but I do not know how.

DISAPPOINTMENT. V. Organic and verbal. I said Oh. My muscles had been strained, but were now relaxed all over. Sensation from frowning. Very unpleasant.

DISGUST. O. Organic sensations throughout the body. Unpleasantness.

DISSATISFACTION. O. Reported twice. Once not analysed. Once reported as consisting of muscular strain, organic sensations, and unpleasantness.

DISTANCE AND DIRECTION. MC. In almost every experiment this observer reports a sense of distance and direction towards a place that is thought of. The two usually occur together, though sometimes the one is reported alone. "Direction goes on in front of my head, and it feels as

if something inside of my head were moving in the given direction. I think this is an image, not a real movement." "Distance is a visual image of the space, boiled down, which lies between the two places. It is a schematic visual image." In one case, in which the observer had been thinking of, picturing, scenes in the east, these were replaced by images of places in her home town, and she says: "In going from one to the other my mind distinctly moved west. This movement is located in the top of the head, and seems like a movement of something inside of the head." In some cases MC says her mind 'flew' to a certain place, and this experience seems to have been a seen motion across a visual area with some kinæsthesis.

EASE OF RECOGNITION. G. Localised once in the trunk, and again as vaguely organic.

EASINESS. (This is partially analysed by O fifteen times, and is mentioned without analysis four times.) Agreeableness, and motor tendency to say Easy.—Motor tendency to say Quite easy. Faint but massive organic sensation in background.—Verbal idea, This is easy, and visual image of my smiling face. — Bodily attitude and relaxed muscular sensation. (In the remaining analyses the same factors are repeated.)

EXPECTATION. Analysis agrees with those of Pyle's observers (this JOURNAL, XX, 1909, 530 ff.).

FEAR THAT I HAD REACTED TOO QUICKLY. F. Slight sinking in the stomach or diaphragm. Lack of clearness of perception, and sensations from breathing.

V reports a state which she calls FRIGHT, and which usually occurs at the beginning of an experiment. This is analysed three times as consisting of a shiver down the back, and a shrinking backward. Once it consisted, in addition, of holding the breath and gasping; and once it is described as a feeling of nervousness and a shiver. The state of SCARE, which might be supposed to resemble fright, is reported only once, and is described as a sudden muscular contraction. C twice reports *fear* as an unsteady organic sensation.

G-CONSCIOUSNESS is reported by G three times. Once it is not analysed; once, said to be motor; and once, to consist of the muscular setting of the tongue.

HAVE FINISHED MY WORK. O. Bodily attitude which cannot be localised. Tendency to straighten up the body.

The I-CONSCIOUSNESS. C. Was a kinæsthetic sensation in the back of the mouth.

I OUGHT TO KNOW THAT. O. Organic sensation and disagreeable feeling.

IMPATIENCE. O. Frowning, and the verbal idea Let 's go.

INJUSTICE. V. Gasping for breath. I started back and threw my head back.

IRRITATION. O. Sensation from frowning. Visual image of frowning face. Tendency to lower and shake the head. Hot sensation in head and back. [In the four rather full analyses that follow, the above facts are repeated, and in addition strain in back and neck (three times), disagreeable feeling (three times), organic sensation throughout the body (three times), verbal ideas This is hard (once) are reported.]

IT MUST BE C. G. A pleasant touch-motor complex, with a horizontal movement, and a downward movement at the end. In the same report, this statement was followed by the attitude *I had C before*, which was verbal-kinæsthetic. The articulation was inhibited. This again gave place to the attitude *That is C all right*, which was a touch-motor complex with clear articulation.

MEANING. G. A single dot below the line means a broken T. This is a motor complex. O. Kinæsthetic and auditory image of the letters. Meaning carried by the shape of the figure itself.

The MEANING OF C is reported by G as an attitude, and is analysed as a complex of tongue-sensations and temperature in the mouth. Tendency to say the letter; pleasantness.

NEWNESS. F. "It impresses me as a cutting edge, as a bit of a knife blade turned up. Definite organic sensations. The whole complex I should call newness."

NON-RECOGNITION. F. Includes vague visual image.

The PASSAGE OF TIME. V describes this three times as a perception resembling that of air currents, passing around and over the observer from back to front; in one case there was the verbal idea That was slow. O says that a feeling of length of time came in kinæsthetic terms, and resembled fatigue.

PASTNESS. HC. The image was projected back into the past. Pastness is more visual than anything else. It seems far away, and foreshortened.

POWERLESSNESS. HC. This was a peculiar tingling sensation all over the body, with sensations similar to fatigue, and warmth.

PRIDE. O. Slight tendency to straighten up my neck and smile. Pleasant feeling.

The READINESS TO SAY A CERTAIN WORD is in one case localised by G in the lower part of the head, and again analysed as a disposition or set of the muscles of the mouth.

HC. REASONING that, if Canute was connected with 1026, Alfred was in the 10th century. This was partly represented by a visual schema of the centuries. Canute was in white at 1026, Alfred in dark, therefore in the 10th century.

REFLECTION. O. Thought turned alternately from A to B. (No further analysis.)

RELIEF. G. Pleasure, and disappearance of organic complex. Pronounced exhaling. O. Consists of organic sensation.

SATISFACTION. C connects this with pleasantness. O. Instantaneous pleasant feeling; faint visual image of my smiling face. Organic sensations. —Pleasant feeling, tendency to smile, organic sensations.—Tendency to smile. Verbal ideas: Yes, it is quite sure. (These analyses are repeated a number of times, while there are also cases in which satisfaction is named but not analysed.)

SECURITY. V. Long, easy breathing and straight position of body; relaxation.

STRANGENESS. F. Weak organic feeling all over the body. Strain, and sensations from breathing, and special sensation from diaphragm. This last is not a strain. It must be one of the sensations that we get when we fall in dreams. O. Insufficient adaptation of body, fingers, and hand. General inhibition, which was a kind of organic sensation; inhibition of arm-movement.

TENDENCY TO STAY AND FIND SOMETHING ELSE. O. Consisted of bodily attitude.

Conviction THAT I WAS RIGHT. V. Reappearance of the verbal image of the letter to which I had reacted.

The attitude THAT I SHOULD HAVE PRESSED SOONER, V describes as a twitch in the finger, organic sensation, and catching of breath.

The conviction THAT IT WAS NOT ANY OF THE OTHERS. G. An inhibited motion of the lips to say B, and the fact that I did not move horizontally and therefore it could not be H. This was partly kinæsthetic, in the fingers and mouth.

THAT IT WAS PROBABLY NOT G. V. Verbal ideas, and visual and kinæsthetic images, of the other letters.

THINKING OVER. O. Consisted of bodily set, and of associations which were not perfected. D and J were repeated several times in auditory terms.

UNEASINESS. O. Reported four times. Once identified with uncertainty. Once includes image of frowning face, and sensations in back of head and neck. Once is a verbal idea, That has no meaning. Once is not analysed.

UNFAMILIARITY. G. The touch-motor complex was unfamiliar and unpleasant. It led to movements of tongue and lips in whispering: Oh me, that must be H. Organic attitude in trunk; unsteady shutting of eyes and horizontal movement of eye-balls. V. Rapid mental comparison of feeling of C now with feeling of C last year, though I do not remember how C felt then.

The UPPER ROW WAS TOO SHORT FOR J. O. Tactual memory of J. Bodily attitude.

WAITING. O. This is described twice, in almost exactly the same words. Sensation of heat in back, stamping of feet, and extreme unpleasantness.

WONDERING. V. Once not analysed; once organic attitude, holding the breath and strain. HC. Once merely reported; once described as a complex of sensations in the top of the head.

ATTITUDES NAMED BUT NOT ANALYSED. Besides the attitudes named, there are others which are merely reported and not analysed. They include one case each of identity of self with the lever (G), the motive to distinguish the dots (G), the consciousness that I had reacted (G), the C-consciousness (G), not being right (F), curiosity (O), relation (O), that has no meaning (O), indecision (O), consciousness of an obstacle (O), unknownness (V), fast reaction (V), contrast (MC), sense of past time (HC); and two cases each of certainty (V), wondering (MC), and ownership (MC; this was at least partly visual).

Many of these incidental analyses are, evidently, imperfect. Even at the best, the observers might report in large and sweeping phrases, such as 'bodily attitude' or 'organic sensation.' There is, however, no doubt that the reports were intended at the time to represent the attitudes themselves, and not merely incidental or concomitant occurrences. After the second set of experiments, the observers were confronted with an outline of their reports upon various attitudes, and were asked to say whether, so far as they could remember, the analyses read were, as analyses, correct. The regular answer was that they were correct, and in several cases the observer added, of his own accord, that he could reproduce the attitude, at the moment, and that it corresponded with the analysis given.

While, therefore, we readily admit that introspective analysis might, under more favorable conditions, have been pushed further, we believe that the descriptions given are reliable, and that more detailed work would simply resolve the complexes mentioned into their elements.

3. *Gradation of Imagery*

Messer finds five stages in the *Entfaltung* of visual images: (1) mere spatial direction or externality; (2) a trace of visual localisation; (3) vague and schematic images; (4) images just named, and not characterised as to degree; (5) very

clear and strong images. In addition, he distinguishes general and particular, partial, changing and moving, and symbolic images. The stages found in the observers in our own experiments are as follows:

P mentions only two stages, and these were only in visual and verbal imagery. The two stages were the vague, and those merely named.

F distinguishes the following stages. For visual images, mere localisation, vague, and just named. For verbal images, which do not lend themselves to exactly the same classification, very vague, just named. Auditory images fall into the same two classes. The idea of movement, which is both visual and kinæsthetic, is very often merely named. Organic sensations were faint, and just named.

V. For visual images: faint, just named, very clear, partial image, change, and movement. For verbal: faint, just named, and very clear.

MC. For visual images: vague, schematic, very clear, partial, change, generic. For verbal: vague, just named, very clear, aloud, partial. For auditory: very vague, just named.

HC's images are in general much like MC's. Visual: localisation vague, just named, very clear, partial, change. Verbal: very clear. Kinæsthetic: vague, just named. Motor: very clear. Auditory: vague, just named.

O has for visual images: localisation, vague, just named, clear, incomplete, movement. He also speaks on several occasions of 'initial associations,' which seem to have been inhibited tendencies toward the articulation of words. For verbal: faint, just named, very clear, aloud, partial. Auditory: same as the verbal, and usually connected with them. Organic: faint, just named, very clear, changing, movement.

G reports for visual imagery only three cases; for kinæsthetic other than verbal, five; and these are all described as vague. His organic sensations are mostly merely named, but a few are faint. On any page of G's reports the verbal stages just named, whispered, and aloud, may be found; yet there is an unmistakable general tendency which, as in the case of V, is in the opposite direction to the order here given. G invariably read the letters aloud when they were being felt for the first time. As they recur again and again, the verbal ideas involved in recognition fade out, from whispered to vaguely verbal, and finally to a mere setting of the mouth or right breathing for the utterance or a certain letter.

Illustrations of the Stages of Imagery.

These typical illustrations of visual imagery represent a large body of similar cases, which cannot be quoted for lack of space. No one of the

observers was instructed to report on the intensity of his images; the experimenter had not this specific problem in mind at the time when the introspections were taken. As the illustrations show, it is extremely difficult to draw a sharp line between the consciousness which is rich in images and that which is 'imageless.' Images may be so vague as to be noticed only by the most careful introspection; and there is every reason to believe that some may escape notice entirely; that, in certain cases, the introspections even of trained observers may not be complete.

MC.

Vague. Vague image of a Massachusetts street. Vague image of a drug store. *Schematic.* Visual schematic image of June in my representation of the months. An image of an environment, a sort of mist, but very vague. *Very Clear.* An image of the girl, including details with much distinctness, such as her light, fluffy hair, bright pleasant eyes, and short dresses.

Partial. Detached image of a face with no setting. Image of a man in the rigging of a ship, but no setting, even of a whole ship. *Change.* Image of the man stepping into the room from the street and looking at himself in the glass. This picture was painted before me. It was not distinct at first, but one part became clear and then another, till the whole was clear.

O.

Localisation. Localisation of the letter in the alphabet slip. (This is repeated many times.) *Vague.* Vague pyramidal image of something. *Just named.* Image of H in point. *Clear.* Clear image of the letters. *Incomplete.* Incomplete image of a Japanese key. *Movement.* Image of a worm moving.

V on the recognition of A

The illustrations begin at the point where color is first associated with A. Oct. 22: Visual image of A written in red; Oct. 26: Very brilliant red; Oct. 28: Vivid flash of red; Oct. 30: Visual image of red A in front of me; Nov. 2: A red flash and a little visual written A; Nov. 4: Visual image of A in front of my eyes, and a flash of red; Nov. 6: Faint flash of red; Nov. 6, later: I visualised the two dots; Nov. 9: Slight red blot. After the reaction, a faint visual A. (See p. 234.)

To demonstrate a series of gradual steps from clear images to 'imageless' thought may not prove conclusively that the latter is a fiction; but, at any rate, it suggests an origin and derivation. Imageless thought, as its name implies, is, so far at least, an entirely negative concept. We perform acts of thought in which we can discover no imagery in consciousness, and hence we infer that thought may be 'imageless.' Just what an imageless thought is like, or how it differs from that in which images are present, has never been shown, or even attempted to be shown, except by way of classifications such as that of Bühler. In fact the term 'imageless' is unfortunate; it lends itself easily to misunderstanding. 'Image,' in popular parlance and often in psychology, means a centrally aroused representation of a visual impression. We must, of course, recognise also the place of auditory, tactual, and kinæsthetic imagery, and theoretically of images of taste and smell, though these appear to play but a small part in consciousness. The verbal image, which is especially important

in the thought-process, may itself be visual, auditory, kinæsthetic, or mixed. Even this list, however, does not exhaust the contents of thought. It is becoming increasingly apparent that our conscious states take much of their peculiar quality from the organic sensations, the felt bodily adjustments, which enter into certain situations. The affective processes are also often present, even in thought. It is obvious, therefore, that the term 'imageless' is inadequate to designate a thought from which all these elements are absent. Even the term 'non-sensory,' which is better, disregards the part played by feeling. We all know in a general way, however, what sort of state is meant by 'imageless thought,' and it is just this state which the above introspections tend to discredit.

For they show that imagery does not need to be specific and elaborate in order to carry thought. We may repeat an argument, or review an article, in a very small part of the words used in the original, without sacrificing the essentials of the content. When we are thinking in verbal images, it is by no means necessary to say every word which we should use if we were talking aloud. Again, just because we are not speaking aloud, it is not necessary that every word should be clearly articulated or completed, either in actual throat movements or in images of such movements. The reports of G, especially, show that the mere setting of the mouth, or the right mode of exhalation, serves as well as the complete word. A young child just beginning to read gets no meaning from his words unless he reads them aloud. Thought consists for him in the sound of his own voice or that of another, or in the feeling of the throat movements involved in speaking. When he first learns to read quietly, he whispers; then he confines himself to a mere lip motion; and later, he can dispense with this expression. The reports of our observers take up this progress where the child leaves off, or from an even earlier point, when the words are still spoken aloud, and show that and how the suppression of sound and the shortening of the total process may be carried in the adult to a stage yet more remote from the starting point.[1]

Detailed Analyses

1. *Explicit*

The attitudes so far analysed have been those which occurred only a few times. It shall be our task now to consider

[1]We are able, owing to limitations of space, to print only the few samples of visual *Entfaltung* given in this section. We have a very considerable amount of introspective material bearing upon the gradations, not only of visual, but also of auditory, kinæsthetic and verbal imagery, and of organic set or adjustment. We hope to present this material in a later article.

some of the more common attitudes in greater detail. One of those which is most often reported is SURPRISE. Surprise, of course, may have various occasions, and whenever these are stated by the observer, they are given in the following quotations.

G. Surprise at the smallness of the letter. Kinæsthetic adjustment, or inhibition of movement, in finger and back.

That I did not feel the letter. Organic sensation, involving inhibited breathing and fixity of the diaphragm.

At not feeling the letter. Something moving along inside the body, a dull pressure going upward from the stomach to the back of the mouth.

Check of horizontal movement and change to vertical.

Peculiar movement of mouth, dropping of jaw, and slight sensation in chest.

Inhibition in diaphragm and setting of the mouth to say Oh. [This last is repeated several times in almost exactly the same words. In two cases the attitude is not analysed at all, and in others it is merely localised.]

O. Muscular strain and visual image of a working nerve. Unpleasant feeling.

Change of position. Organic and muscular sensations somewhere, and blank consciousness.

Relaxation of muscular strain, and a tendency to change the bodily attitude.

V. Holding of breath and kinæsthetic sensation at finger tips.

Running of warm currents all over the body, and tingling sensation. Very pleasant.

Verbal exclamation.

Kinæsthetic and verbal. I dropped my jaw, and opened my mouth. Sudden muscular contraction, a gasp, and turning the head.

Muscular contraction.

Held my breath and gasped and contracted muscles.

On comparing these reports, we find that surprise is, in general, an inhibition of movement, or change in its direction, often accompanied by sensations from inhibited breathing, organic sensations, verbal exclamations, and affective processes. The important thing seems to be the change or inhibition of muscular reaction. Surprise may be called the conscious aspect of the adjustment to a new and sudden event in the environment.

The consciousness of SEEKING for something, of trying to remember, is called by Marbe a *Bewusstseinslage*, and is classed by Messer as an affective attitude, in which the relation of the *Aufgabe* to the object recedes, leaving a subjective state for whose solution the *Aufgabe* suffices. Watt affirms that the consciousness of seeking for the reaction-word is not present in every experiment; when present, it consists of a consciousness of direction, and an emptiness of consciousness, with a repetition of the *Aufgabe*, and sometimes a visual image. Orth says that the attitude of striving to find is a complex of organic sensations, bound up with the representation or immediate consciousness of a goal. Messer assumes an at-

titude of seeking, which is unanalysable, but finds with this in the whole situation motor processes, movements of the head and eyes or representations of such movements, organic sensations, strain and obstructed breathing.. All these components may not be present in a given case. In his "Snap Shot of a Hunt for a Lost Name," Bailey finds three stages, which include visual images, kinæsthetic and organic sensations, and pleasure, beside several complexes such as belief.[1]

As the experiments on point-letters were not suited to bring out this attitude, special introspections were taken. The observers were MC and HC. Some of the analyses occur in experiments given for another purpose, and in a few cases the occasion for trying to recall arose incidentally, and the descriptions were written down immediately. Since these cases did not promise to be very numerous, however, a special method was devised. The observer was shown an old and almost forgotten picture of her classmates in the High School. All of the picture was covered except one face, and the observer was asked to recall who the person was. Whenever the response was immediate and no seeking was required, the case was thrown out, and another face exposed.

MC. Visual image of the face, and verbal idea of the name X. In trying to think of the right name, I experienced a kind of working or agitation localised in the top and back of the head. Several names came to consciousness, and visual images of persons that I associate with this girl.

Visual image of the picture and also of the girl. The first glance brought a verbal image of a name XY, followed by images of XY in two situations. I have also a feeling of time, that is that XY was much farther back in time than the subject of the picture. I do not know what this is. It seems to be chiefly a series of instantaneous pictures.—

In other cases, events and scenes which might suggest the name are recalled visually, laughter and voices are heard in image, and even the peculiar carriage and gesture of the person are imaged by the observer. In most of the cases recorded the attempt to recall was unsuccessful. The reports contain also frequent reference to sensations from squinting and closing the eyes, sensations of 'drawing' in the top of the head, 'emptiness in the head caused by the inability to recall the name.' Again: "The brain working seems in this case to be in the front of the head instead of the back as before. Sensation in the eyes and vaguely in stomach. Feeling of unpleasantness caused by inability to remember."

The sense of seeking for something occurs about thirty times in the same observer's later introspections upon the meaning of words, and is invariably described as a groping feeling in the top of the head, with sometimes a knitting of the brows, wrinkling of the forehead, squinting of the eyes, and sometimes a visual experience which is described as staring into a blank field. Occasionally the object of search finally appears as a small object in the middle of this field. Consciousness seems less rich in content in these latter reports than in the earlier.

The introspections of HC were obtained under exactly the same conditions as those already discussed. The common element in them all is sensation from the diaphragm, though that from head and eyes is often

[1] *Journal of Philosophy*, etc., iv., 1907, 337.

present. In the cases of seeking which occurred incidentally, the strain is very often in the eyes and forehead. Visual images abound, while kinæsthetic and auditory imagery is sometimes reported.

It is evident that the consciousness of seeking consists of strain in the head and eyes or internal organs, and a feeling of effort localised in the head, together with images of any kind which have any connection with the required fact and would be likely to recall it.

The attitude which occurs most often incidentally is DOUBT. No sharp line can be drawn, it seems, between doubt, uncertainty, and hesitation. The term used by the observer has, however, been preserved in the quotations, so that, if there is any inaccuracy in this interchangeable use of terms, it affects the deductions alone, and not the results from which they are drawn. It is certain, at all events, that in many cases the observers themselves did not distinguish the three attitudes, and that the same kind of analysis is given under all three names.

Doubt is one of the attitudes most often named by Marbe, Orth, and others; and in Ach's separation of *Bewusstseinslage* from *Bewusstheit*, it is included in the former class. It is assumed in general, all through this series of papers, that doubt cannot be analysed. Thus, Orth finds doubt accompanied by sensations in the head, organic and kinæsthetic sensations, images and feelings. "Doubt is not a feeling, but a complex state, whose constitutive element is the *Bewusstseinslage* peculiar to it." Here is really an analysis of doubt, *plus* the assumption that the analysis is incomplete, and that there is a central thread running through the experiences, which is indescribable except by the phrase that it always characterises doubt. Whether or not there is such an element is a question for introspection to decide.

The following accounts are taken from the introspections of G. *Uncertainty.* Slight organic excitement in the abdominal region. Verbal ideas: 'Well, what is that? The movement changed from the B to the C movement, which was actually repeated. Long dwelling on one impression. The disturbing other dot was always there. There was nothing pleasant after the reaction, because I was not sure.—Suspension of verbal association, or inhibition of movement in the mouth. The mouth did not set itself till the recognition was complete.—Verbal idea: What the deuce is that? The letter seemed too short; inhibited breathing, and pressure in larynx.—*Hesitation.* Hesitation between B and J. I set my mouth for B, then for nothing at all. Renewed touch-motor complex.—Familiar, then unfamiliar aspect. Kept finger on letter and pressed harder on last dot.

F. I was not certain. It did not feel quite right. I compared it with the tactual images. The perception had a blank place in it which did not belong to H, and the real H was in this space.—It was not a clear cut G and did not come forcibly.—*Doubt.* A came, but it was not exactly as it ought to be; E came, but I could not decide which it was. Strain from hesitation. I was not sure. Kinæsthetic sensation high up in arm. Not being sure was not anything in consciousness; I just did not react.—*Hesitation.* It

was not clear, and I kept feeling it. It was not H because not bulky enough; I said J, but it did not seem to be J because too big. I said C, but it was too big for C. Visual image of a triangle. J came, but it still seemed too big.—Kinæsthetic image in forearm and finger. Tactual sensation not very clear. I did not react.

It will be noticed that the element common to almost all of these cases is one which may be called negative: the failure to move on, and to complete the reaction. When there is no doubt, the process proceeds without interruption; but in doubt there is "long dwelling on one impression," "the finger is kept on the letter," there is "renewed touch-motor complex." In addition to this, there is, especially in hesitation, a vacillation between two tendencies; two aspects alternate in consciousness, or the mouth is set to say now one letter and now another, without saying either. Verbal ideas and organic sensations may be present. Doubt is connected with a lack of clearness of the tactual perception. As the reaction is delayed, the attempt to recognise is repeated, and the reaction is prepared for again and again.

O. *Uncertainty.* Swift succession of auditory-kinæsthetic verbal images, initial movements of tongue, and faint sounds. Queer sensation from bodily and facial attitude. Visual image of my face full of confusion and unpleasant feeling.—*Doubt.* Several initial associations, consisting of sensations from irregularly contracted muscles, and a visual image of them; unpleasant feeling, organic sensation above abdomen, and sensation from irregular breathing. Interruption of breathing, organic sensation in chest, and pain in back of neck. Organic sensation in back and spine.—The same, with verbal images.—Organic sensation in head, chest and abdomen, unpleasant excitement, faint visual image of my vexed face, organic sensation in both shoulders, muscular strain in neck.—Auditory-kinæsthetic images of A, and something unclear tried to come into consciousness but was inhibited. Organic and muscular strain in head and chest. (This is practically repeated several times.)—Initial tendency to produce B, C, or D. Faint verbal and auditory image of J, then "it can not be," with organic sensation. (This last is often repeated, sometimes with unpleasant feeling.)—Unsteady and recurring organic sensations, which continued a long time.—Organic sensation like heat in the brain, holding of breath, muscular sensation and pain. Absence of relief, the muscular strain persisted. Verbal idea "I might be mistaken." (Almost every introspection given here is reported a number of times. There are three unanalysed cases of doubt.)—*Hesitation.* Verbal image of D and C alternately. Verbal idea that the dots are very strange, disagreeable feeling, sensation in back. (This is essentially repeated several times.)—Tendency to stay and find something else. (Hesitation is once unanalysed.)

O's consciousness is made up very largely of organic sensations and strains, and shows much more affective coloring than those of G and F. Doubt and uncertainty were unpleasant and irritating.

P gives introspective accounts of the attitudes of doubt, uncertainty and hesitation; the analyses agree almost completely in being verbal. P's consciousness is so strongly verbal that whatever else was present seems, in most cases,

to have been so far in the background as to be overlooked. There are a few references to organic and kinæsthetic sensations, unpleasantness, holding of breath, and general strain. The fingers were sometimes moved over the letter again and again.

V's analyses of uncertainty are meagre.

I frowned and held my breath.—Unpleasant and annoying.—I could not tell the number of dots. I tried to count them separately, but finally had to rely on the image I got from the whole sensation.—*Doubt.* The dot seemed flat and did not fit well into the finger.—The letter did not fit. I held my breath and muscles stiffened. Muscular strain and verbal idea, Perhaps it was D.—Frowning, vague organic complex, and verbal idea: I am not sure. (These are all repeated several times. For V the doubt in these recognition experiments usually came after the reaction, so that it could not consist in the inhibition of reaction. V's recognition, whether right or wrong, was usually immediate, and the reaction time was very short. In the few cases in which doubt occurred before the reaction, the time is increased above the average. As the feeling of fitting on the finger is the most common element in V's recognition, so the absence of this fitting most often marks doubt, but it is very often accompanied by an inhibition of breathing, muscular strains, and verbal ideas of doubt.)

Doubt does not occur in MC's reports, and occurs only three times in HC's. It is here analysed as a feeling of displeasure, organic sensations from stomach and diaphragm, and alternation of attention from one visual image to another.

Although the observers differ strongly in type, it is easy to see that there is a general agreement in the analyses of doubt. It involves an inhibition of the reaction, a checking of the habitually smooth-running process. So far, it might seem that doubt and surprise are alike; but there is a difference. Surprise involves a sudden checking or changing of the motion begun, whatever it is. Doubt tends to check the reaction, if it has not already taken place,—to lengthen the reaction time. The motion involved in feeling the letter, however, is not checked, but is prolonged or repeated. The observers of verbal type have verbal ideas of doubt; others have organic and strain sensations, and often unpleasantness. There is a marked tendency to use the term *hesitation* when there is a vacillation from one word, image or form of reaction to another. The organism meets a situation to which it is not prepared to react promptly. This may be because a particular reaction is begun, and then inhibited, or because several are initiated in swift succession, and inhibit one other. We may recall Washburn's derivation of the feeling of "but." The incipient movements of developed consciousness, in which the mouth sets itself to say now one word, now another, or the hand to make this or that reaction, may be vestiges of larger movements following one another in swift succession and tending to carry the body in opposite directions or to set it for incompatible acts.

2. *Genetic*

The fact of development has already been recognised by the writers on attitudes, though its whole bearing upon the question has apparently not been seen. This statement holds in particular of the case of the *Aufgabe* or task. Marbe reports a *Bewusstseinslage* which the observer called 'memory that it must be answered in a sentence,' and again one that the observer called 'recollection of the problem.' The first of these is referred to, with approval, by Orth, and is placed by him in the second group of his classification. If we turn to Watt's work, in which the course of the experiment is divided into four parts, we find that the *Aufgabe* is not only clearly recognised, but is also analysed and genetically developed. Watt is specific upon the point that the task (or part of it) gradually drops from consciousness as the series proceeds, but that the degree of consciousness of the preparation has no relation to its effectiveness. Messer is, in this matter, in entire agreement with Watt and Ach.

The course of our own experiments was at no time divided into parts, and in general little attention was paid to the consciousness of the task. Sometimes, however, voluntary reports were made, and at other times the observer was asked, at the time of introspection, if the task had been present before the reaction. The instructions were not repeated at the beginning of every hour. At the time, this fact did not seem worthy of notice; but we believe that they were never specifically repeated, to any observer, after the first hour of work. The instruction to recognise a letter is so simple as not to need repetition. The letters in alphabetical order were usually given to the observer at the beginning of the hour; but sometimes he reported, as we have remarked, that he did not need them again. This reminder may have served as a renewal of the general consciousness belonging to the task. Again, the general position of the body must be resumed at every sitting; that is, one finger must be on the reaction key, another at the edge of the card holder, the eyes shut. In so far as the *Aufgabe* is carried by bodily position, it must always have been present.

The TASK is specifically reported in a few instances.

G. Verbal ideas: I must make up my mind to react, and an uncomfortable bodily attitude, very unpleasant. (This was not reported as a part of the fore-period. The recognition and reaction seem to have been delayed, and the task is recalled in order to hasten reaction. This occurs in the second experiment of an hour, the ten letters having been felt at the beginning.) In a second case, the task is analysed as the organic setting in saying: Hurry up, don't be so long. (This too is a case of delayed reaction, and is the seventh of a sitting, the letters having been felt at first.) V reports the task to press the button as being present in kinæsthetic terms. This

is in the fourth experiment of the first hour of the second series. The recall to consciousness does not seem to have been occasioned by any difficulty. In the third reaction of the next hour V "forgot the *Aufgabe* to react," and therefore did not react as soon as she recognised the letter. The whole present purpose was to recognise the letter, and this purpose was a muscular adjustment, and concentration of attention on the finger tip. In the second experiment of the next hour the task was present in motor terms, and especially in a strain sensation in the finger, which relaxed after the reaction. F reports what he calls the "hurry-up" consciousness, and again the *Bewusstseinslage* that I must do it quickly. The former is analysed thus: Strain became noticeable in the abdomen, as if two sensations came from the sides and met. The "hurry-up consciousness" is mostly strain. Again: Organic sensations from diaphragm. The muscles of the diaphragm seem to come up and press the lungs, and the muscles of the ribs seem to tighten. (This always comes with the strain of waiting.) Again: Strain when I heard some one walking. The strain meant "hurry up." (This is often repeated in the same terms. The "*Aufgabe* to do it quickly" is described twice as consisting of muscular strain.)

Nearly all these cases occur early in the second series, and the *Aufgabe* is not later reported by name. It does not follow, of course, that it was not in consciousness at any other time, or that it was not reported in its elements. The conscious contents occurring before the perception of the stimulus were often complex and important, and in some cases occupied much more space than the rest of the introspection.

In the early part of the series O reports sensations from the different parts of the body, as the bending of the neck, stamping of the feet on the floor, pressing together of the teeth, and auditory images of the sound made by the finger in moving over the paper. About the middle of the series he several times remarks: "I did not notice the bodily adjustment of attention," and in the last thirty or more experiments the introspection usually begins with the perception of the letter. If there is anything before this, it is usually the auditory sensation from the rubbing of the fingers over the paper. The strained condition of the body is mentioned only once in the last thirty cases. In the reports of F, also, we notice a marked falling-off in the number of conscious contents occurring before the appearance of the stimulus. The first introspection is almost entirely an account of the fore-period, and includes strain, breathing sensations, kinæsthetic, temperature, verbal and visual images, some of these occurring several times. In the experiments immediately following, the contents of this waiting period are only slightly decreased. Later the observer reports: Kinæsthetic and visual images of moving fast over the letter. Later, again: The 'ready' set me off without a conscious *Aufgabe*. The idea of movement with the tactual image is repeated a great many times, but toward the last is described as vague, and does not appear at all in some of the latest observations of the series.

The conclusions to be gathered from these data are in entire agreement with the findings of Watt, Ach, and Messer, who offer us not only avowed analyses of the *Aufgabe*-consciousness at the beginning of a series, but also repeated proof that it is shortened and modified and tends entirely to disappear. If any observer were stopped just before a reaction, and asked to state in words what he was about to do, he would doubtless be able to reply. We may reason, logically, that he could not state what he did not know, and that here is therefore a case of 'imageless' knowledge. It

may be answered: (1) that such a statement would be a clear case of *Kundgabe*, a report of the significance of a total state and not a description of contents; (2) that, even if such a report be conceded to be the verbal expression of an 'imageless' state, it is nevertheless shown to have derived by direct development from a consciousness whose contents could easily be isolated; and (3) that, so far from any indication that the *Aufgabe* was present in other than sensory terms, it is specifically said not to have been consciously there at all, and yet to have been active. The reports throw us back upon unconscious tendencies, but not upon an unanalysable attitude or an imageless thought.

The attitude which in our experiments shows best from a developmental point of view is RECOGNITION. From the nature of the point-letter experiments, it follows that every introspection is really an analysis of the recognitive consciousness. It may not be complete, and it may on the other hand contain experiences that were incidental, but in the main it represents recognition. The letters were absolutely new to the observers when the experiments were begun, and when they were ended most of the letters were recognised both accurately and promptly. Our conclusions will, however, be based chiefly upon the second series, in which the time was measured. To part of the observers the letters at the beginning of this series were not entirely unfamiliar.

It is to be supposed *a priori* that, as the experiments proceed and a letter is given again and again, it becomes more and more familiar. This inference is supported in a general way by the curves of accuracy and of time, which show that on the whole all the observers make fewer mistakes toward the end of the series than toward the beginning, and that the time required for reaction becomes progressively shorter. Sometimes, it is true, cases of false recognition or of long reaction-time occur at the end of a series. These can usually be explained, however, as due to accidental causes. For example, the raised letters tended to become worn off, and to grow less distinct with repeated rubbing. Care was taken that they should be replaced before this injury occurred, but sometimes it escaped notice. The introspections enable us to trace most of the irregularities to these and other causes. Even without this allowance, however, the curves show a striking similarity of form, and, with one exception, an invariable tendency to decrease both of mistakes and of reaction-time as the series advances. Curves of the length of the introspections, in lines or words, would show the same general slant from left to right. Of course, such a result could have only the most general application, but the differ-

ence in length of introspections is striking, and, taken with the other indications of shortening, has a certain significance. At the beginning of any series there are usually one or one and a half introspections to a type-written page, while at the end they average from five to ten, according to the observer.

The letters chosen, which were the first ten of the alphabet, turned out to vary greatly in degree of difficulty. E, being a single point, was the easiest of all. F, I, and A were also easy, while G, D, H, and J were difficult, and B and C stood midway. The intermingling of these difficult and easy letters brings irregularities into the times, and makes it necessary to study the letters separately.

The letter which gave O the most trouble was D. The average time for D was longer than that for any of the other letters, and the letter was usually not correctly recognised. In fact, it was called D only once, the last time that it was given. The reaction-times are very irregular, increasing gradually from the beginning, suddenly dropping about the middle of the series, and then again increasing. At first D is confused with G, but later consistently with J. The number of elementary experiences reported varies roughly with the reaction-time. When D first occurs (D. j, 4.42), the introspection includes perception of shape, organic sensation, disagreeable feeling, and images. In the next case (D. g, 8.40) the image of the reaction-letter comes verbally as well as auditorily, and there is doubt. Otherwise the report is essentially the same. In the third case (D. j, 11.62), consciousness is still more complex, and includes repeated perception, more complex verbal images, and several visual images, of which some are merely associative. In the fourth (D. j, 10.24), there are added frowning, irritation, organic sensations throughout the body. "Very disagreeable. So many sensations and images that I cannot remember them all." In the next, there is a marked decrease. In the next three or four, there is increase again; and in the final case (D. d, 15.42), the observer says: "Auditory image J, or D. I thought it over again and again. This was bodily setting, and associations which were not perfected. The whole was accompanied by strains and organic sensations, verbal and visual images, and unpleasantness, especially at the last." Evidently the observer himself was not satisfied, and the reaction to D was not a true recognition.

This series will serve as an example of the correspondence between amount of content and reaction time. It is evident that we have not here before us a growth of the recognitive consciousness. As the letters in alphabetical order were given to the observer at the beginning of the hour, he could not form a closer and closer association between the letter D and the name J, which the repeated wrong reactions might otherwise have caused. So we find irregularities, with a tendency toward increase rather than decrease. The result shows, so far as one case may do, that the shortening to be observed with increasing ease of recognition is not to be attributed to a general habituation to this form of experiment. The change due to habituation can be observed, but it occurs mostly in the fore-period, and it is not sufficient to account for the progressively shorter reactions to particular stimuli.

The letter A, which for most observers was one of the easiest, was for O next in difficulty to J. It shows, however, a different change in consciousness. A is always recognised correctly except in one place, in spite of its difficulty, and consciousness shows a general though very irregular prog-

ress from complexity to comparative simplicity. In the beginning (A. a, 14.78) O reports difficulty of perception, organic sensations in abdomen, spine and back, disagreeable feeling, verbal ideas, auditory-motor images, and doubt, which last was a tactual memory image of the former size of the dot and a comparison of this with the sensation. In an introspection chosen from the latter part of the series, there are only tactual perception, visual image of dots, verbal idea A, and surprise. In this case the letter was rightly recognised, and the time was 2.78 sec. F and E show the shortest times, but they are not very regular. Practice was not carried far enough with O to reach automatic and prompt recognition. With every letter except D there is a change toward simplicity of consciousness and shortening of reaction time.

The letter C was one of the easiest for the observer F. The introspections may be compared with the time curve. C. c, 3.20. The reaction was unnecessarily long, I forgot to react. Tactual images as I moved my finger, especially an image of touching the lever. Sensation of strain in shoulders and chest, slight interruption of breathing. Attention all on tactual images. As I was moving across I had the verbal idea: Tactual images. C is a movement across and down. It is kinæsthetic and tactual. C came up verbally and auditorily. Saying C recalled the *Aufgabe* to react, in what form I do not know. C. d, 3.30. Visual and kinæsthetic image of movement. I said 'as before' while I was moving across. Tactual image of a point. Touched the letter, and I came up verbally. I knew that it was not clear enough; it did not fit into what I know I is. The whole complex was not just right to produce reaction. I felt twice more and D came up. The reaction still seems just like saying the letter. 'As before' meant going over it without attending to the lever. C. c, 2.16. Idea of movement, partly visual and involving eye movement. D came up at first; then I felt the rest of the letter, and C came up verbally and auditorily. The breathing was right to say C. I was trying to catch the verbal image and noticed the breathing. C.—1.96. Strain when I heard some one walking. This strain meant 'hurry-up.' I touched the letter and it was not clear at first. I felt it again. It was familiar, but I could not think what it was. C. c, 2.10. I felt two dots above. Attention on upper dots, then vague perception of lower one, and I moved down to feel it. This motion named the letter. C. c, 0.96. Tactual and kinæsthetic image of feeling the letter. The location of the letter in advance is kinæsthetic and tactual. C was visualised as a dark mark. C.—1.16. I did not recognise the letter. It was very clear. I reacted to the tactual sensation. C. c, 0.62. I set myself muscularly, touched the letter, and moved part way over it. F came up auditorily. I moved the rest of the way and C came up. Reaction and relaxation. C. c, 0.44. Vague tactual image and muscular set. Clear tactual and kinæsthetic sensation. There is an image of the kinæsthetic sensation which enters in and makes a part of the C. When I move my finger straight across, it seems as if I had moved it down. This is an image. Auditory image C after reaction. C. c, 0.36. Idea of movement. I felt it, and the letter C came up.

The reaction times of V are uniformly short, but show a very regular decrease. There is a striking similarity among the curves, some of them being scarcely distinguishable. Early in the series there are mistakes, and some entire failures to recognise the letter. In far the larger number of cases, however, the recognition is unerring and the reaction times are short. It has already been shown (p. 223) that the color images, which accompany certain letters for V, gradually fade out. In the introspections, of which we give but the briefest sample, there is a corresponding disappearance of other conscious contents.

In the case of H, V reacted to a vague mass. The voluminousness seemed to be the one thing recognised; and she recognised the letter in a fraction of a second just from its voluminousness, long before the shape

began to be definite. J was associated verbally at the beginning with the strange thing which did not fit, but this letter too came later to have more definite outlines.

As G had done no experiments in the first series, the introspections here given represent the whole course of improvement from the first presentation of a letter to its almost automatic recognition. I. i, 1.86. At first a touch complex which was focal. Repeated movement in the vertical direction, which led at once to recognition and verbal idea I; movement and verbal idea focal, organic complex vague. Feeling of recognition pleasant. I. i, 2.82. Horizontal movement with complex touch led to no recognition but to vertical movement over the dots. Visual image of I and very pleasant mood, localised in trunk. This might be called ease of recognition. I. i, 2.60. Slight surprise, that is a check of the horizontal movement, and a change into the vertical, with peculiar movement of the mouth, a dropping of the lower jaw, and slight sensation in chest. The setting of the mouth led involuntarily to saying I aloud, and reaction. Last slightly pleasant. I. i, 1. 40. I is recognised by the checking of the horizontal movement and the substitution of the vertical. Setting of the mouth to say I. Reaction automatic. I. i, 1.14. Inhibited horizontal movement. Strong setting of the mouth to say I, which lasted after the reaction and led to saying I. Slightly pleasant. I. i, 2.40. Slight setting of the mouth to say I. Indifferent or slightly pleasant. I. i, 1.16. Different from the usual I-consciousness. Motion not inhibited. Vertical motion replaced by tactual perception of two dots, one above the other. Slight pressing down of the lower part of the mouth, vaguely localised. I. i, 1.60. Movement horizontal, then vertical. Organic and affective inhibition of horizontal movement. As soon as this vertical movement was established, slight pressure of the larynx in setting of the mouth to say I. I. i, 2.22. Touch-motor consciousness, with change from horizontal to vertical very clear. Vague pressure in back of mouth. This was the I-consciousness. Slightly pleasant. I. i, 1.36. Stereotyped I-consciousness. I. i, 1.36. Stereotyped I-consciousness. Kinæsthetic part in back of mouth more pronounced than usual. Auditory image: I again. I, i. 1.60. Surprise. This was inhibited breathing and setting of the larynx and back of mouth Later the common I-consciousness.

There can be no doubt that the introspections given, and the far larger body from which they are taken, represent recognition in the making. In the whole number of reports there are at most only one or two cases of the comparison of a percept with a memory image. There is not a single allusion to a quality of knownness attaching to the perception. The feeling of familiarity is reported thirty-five times, but in all but four it is analysed. The introspections of any observer show, in general, a dropping out of conscious contents, and a shortening of reaction-time going parallel with increased ease of recognition. Only one case was noted in which the process was reversed; the reaction-times here become longer instead of shorter, the conscious contents increase in complexity, and the letter is continually mistaken for another.

The cases of reported familiarity almost all occur in the first half of a series. Their analyses show that they are complex states, consisting mostly of strain and organic sensations, with some affective processes and accidental associations.

Among the final experiments of most series, where we may assume that complete recognition occurs, we find cases in which the reaction is said to be automatic, and the letter may be represented in consciousness by a setting of the mouth to say it, a visual image, a flash of color, or by nothing at all.

If the terms 'familiarity' and 'recognition' are used interchangeably by writers on the subject, it may well be that they were also confused by our observers. We should not lay too great stress upon the names applied to the attitude. With all allowance made for inaccuracy, however, there are suggestions here for the arrangement of familiarity and recognition along a scale of continuously varying complexity.[1]

If recognition has received considerable attention from psychologists, so also has UNDERSTANDING. In fact, the two approach each other so closely in experience that it is impossible sharply to distinguish them. Our own experiments combined the methods of Ribot, Binet and Taylor. The stimuli were words, sentences, and paragraphs cut from magazines, which were read sometimes to the observer, sometimes by her. The stimuli were varied in two directions, from simplicity to complexity by means of the length of the passage to be read, and from sense to nonsense, some of the stimuli being strange or impossible words. If there is a special consciousness of understanding, it ought to stand out, by contrast, in a series in which part of the stimuli are not understood. The experiments were performed during the summer of 1909 with the observers MC and HC. The latter wrote introspections on only 100 single words; MC was given the same number of words and, in addition, 50 sentences and 20 paragraphs. The general procedure was that the observer drew a slip from an envelope, read the stimulus, and wrote the introspection. When the report was dictated, or the stimulus read by the experimenter, the fact is noted. The words used include nouns, both abstract and concrete, and various other parts of speech. In almost every case MC reports verbal and visual images of the word as the first thing in consciousness; and sometimes, if the word is not very familiar or is one whose meaning does not easily appear in visual images, it is repeated several times.

MC. It was invariably the case that, if the word was unknown to the observer, she immediately associated it verbally to some known word of similar sound, and the other contents of consciousness referred to the meaning of the known word. Sometimes only a part of a word was con-

[1] Lack of space, again, forbids a discussion, in this and the following sections, of the views and results of earlier investigators. General reference may be made to Titchener, *Exp. Psychol. of the Thought-processes*. We hope to recur to the theory of recognition, in particular, in a later article,

cerned in this association. *Metappos.* Visual image of Italy and Switzerland on the map, accompanied by sense of direction. Verbal image of the words Matterhorn and Metaphysics. *Frostilla.* Visual image of a frosted cake, then of frost on a window pane. I do not know the meaning of the word, but think it may apply to some kind of extract.

Even when the word was understood, it was sometimes associated to others of similar sound, or divided into parts and then connected with images which referred to but a single part. This verbal association occured most often when the word was a pronoun or preposition whose meaning was hard to grasp apart from context. *Display.* Visual image of the two syllables separately, and of a stage at the theatre. Later visual image of a woman overdressed, not very distinct. *Which.* Verbal image Witch, with visual image of a volume of Scott and of Meg Merrilies. Then repeated verbal image of the stimulus word, with slight groping in mind, followed by image of a page in a grammar.

By far the greater number of meanings were represented by visual images, and these were most likely to occur with words denoting concrete objects, though they were not confined to them. *Compartment.* Image of a train, and of the words 'train' and 'European.' Sense of direction and distance. Visual image of some pigeon-holes.

The contents of consciousness were usually far poorer in the understanding of such words as prepositions, which have little meaning apart from context. *From.* Repeated visual and verbal images of the word. Sense of distance and direction in going from one place to another. Visual image of a country road between two familiar places. *The.* Visual image of the word, then again in big letters at the top of a newspaper; no particular one. Then a blank. I shut my eyes and looked into a blank field. Consciousness of 'seeking,' but nothing came.

Sometimes the setting which the word called up was verbal rather than visual, and sometimes the verbal ideas constituted a definition. *Adjacent.* Word at first seen sidewise and not recognised. The J was most conspicuous, and this came as a visual image, followed by verbal image; Dr. Jekyl and Mr. Hyde. Then visual and verbal image of the word, the latter many times repeated, and a sense of groping and looking into a blank field, though less blank than usual. It seems at times to be dotted with conspicuous black letters such as made up the stimulus word. Later verbal idea: Things close to each other. *Marvelous.* Visual image of a house occupied by Prof. M (name similar in sound to stimulus). Later verbal image: Wonderful. *Rose.* Visual image of a rosebush, and of two girls named Ross. Verbal idea: Roman de la Rose. Images were unusually numerous here, and I cannot recall the others. They seemed to be present all at once.

As might be expected, there are cases reported in which the word was understood, but the observer could not analyse the understanding. There are, however, only seven or eight of these among the hundred words, and in most of the exceptional cases there is some indication that sensations or images were present. *Right.* An immediate consciousness of the idea 'to do right.' "This was partly verbal, but I seemed also to see something of a schematic nature. I looked downward to see this." (Whatever the observer looked down to see must have been of a visual nature.) *Individuals.* Image of a light place (probably an after-image) with the word in it. It all seemed to be inside my head, then I seemed to try to get it out. Paralysed feeling. I knew what the word meant, but it did not suggest anything. Then visual image of individual people walking. They bowed. Knowing what it meant was a kind of comfortable feeling, a feeling that I could define the word. This was in the back of the head and throat. (The feeling that she could define the word, localised in the back of the throat, was probably the setting of the vocal cords to articulate the definition.)

These introspections, like those of Binet, Ribot, and Bagley, show cases in which the images were inadequate or contradictory to thought. *Noiseless.* Visual image of a house, of a very noisy family of boys who occupy it, also an auditory image of the noise of those children. This was accompanied by a sense of contrast, but I am too tired to analyse this. *Electric.* Visual image of a park that I once visited which, however, was not electric. There seemed to be in the background images of several electric parks but the other was more distinct.

In the next series, of 50 experiments, the stimuli were sentences of various lengths. Here, too, the meaning is most often represented by visual images, if the sentence describes a visible object or a scene. In such cases the whole scene may be painted before the observer's eyes, a part at a time, as the words come. *The engineer and fireman and one or two others were standing by the engine staring at it; and so they hastened thither, well ahead of the outpour of people behind them.* (Read by E.) The series of visual images came during the reading. Verbal images of first two or three words. Visual images of engineer, fireman, engine and crowd. *He climbed up the straight iron steps to the gangway.* Visual image of iron filigree work. Verbal image of whole sentence. Image of a person going up a gangplank in the Brooklyn Navy Yards. Second verbal repetition of the whole sentence.

For this observer all sentences, even of an abstract nature, arouse visual images which represent, more or less schematically, the meaning of the whole or of single words. Sentences which are not descriptive may nevertheless be represented visually. *Every vigorous state pursues two principal aims: to enlarge its dominions and to preserve its independence.* Visual and verbal images of sentence. Words 'principal,' 'territory,' and 'independence' stood out most prominently. Even while I read, I connected it with my history work. Visual image of table at which I work, and a 'feeling' that the sentence belonged to me. Visual image of the Mississippi valley, schematic and a good deal in the background. The country was being extended into this valley. Sense of distance and direction to Atlantic states. The word 'independence' meant the Revolution. This was mostly direction and distance, and a schematic image of the Revolution. Independence Hall was included.

Some sentences arouse very little imagery, and yet are understood. *Life is ruled by the power of the deed.* 'Deed' was at first read as 'dead,' and gave an image of Egypt and some mummies. When I read it again, I knew what it meant, but it was not as clear as usual. Slow repetition of the sentence. Emphasis on 'life', 'rules', power', and 'deed'. 'Life' called up an image of a Chicago street. 'Deed' gave an image of myself doing something.

The third series was similar, except that the stimuli were whole paragraphs, varying in length, but all longer than the sentences. Here again a description of anything which could have been seen is very fully illustrated by visual images. In some cases these are added as the words come, and make a whole picture; in others the images seem to refer to individual meanings of the words, and not to the whole situation. *An arduous task must have been that of the first ministers of the Jamestown Church. A part of religious services enjoined were as follows: on week days, early in the morning, the captain sent for tools in place of arms, when the 'serjeant-major' or captain of the watch, upon their knees, made public and faithful prayers to Almighty God for His blessings and protection to attend them in their business for the whole day after succeeding.* Visual image of some ministers. Sense of distance and direction to Jamestown, visual image of the town, and vaguely of a church. At 'religious services,' image of a church. 'Early in the morning' gave an image which seems to grow or develop. First there was what I call the representation of a morning, which was partly visual but included other sensations, such as the pressure of air on the

face. Visual images of captain, tool, army, etc. 'Upon their knees,' image of people praying.—Even when the meaning is abstract, there is still imagery.

The second series shows an interesting case of the process by which a visual image becomes stereotyped and loses its particular quality as it recurs repeatedly in the same connection. Several of the sentences were from an article on Buddhism, and this word called up the same association each time, with some modification, as follows. (1) Visual image of the word Buddhism and of a Dr. X whom I once heard lecture on this subject. Visual image of something brown, which represents India, and contains an idol. (2) 'Buddhist' gave image of Dr. X. A long row of idols and something brown. (3) Image of a sort of conventionalised Dr. X. Any reference to Buddhism always calls up Dr. X, and this has happened so often that the image seems to have ceased to be personal. (4) Visual image of Dr. X; it was rather an image of an image. (5) Schematic image of Dr. X.

The 100 introspections written by HC on the understanding of single words need not be discussed in detail. Some of the reports show that the meaning of a word may be carried, in whole or in part, by a motor image or an organic sensation. *Grip.* Visual image of a hand reached out to grasp something, and muscular image of the sensation in right arm and hand when something is grasped. *Approval.* Image of a person vaguely seen nodding his head and smiling. This was accompanied by motor images of the action, which were much stronger than the visual, and yet the latter was not of myself. *Stroke.* Swift motor image of striking something. *Wanted.* Organic sensation in stomach.

Several of these reports show well how the mind goes from one to another of several possible meanings when the word is presented without context. *Glasses.* Visual image of spectacles alone, then on a person. Then of tumblers on a table. *Reproduced.* Verbal image of the word. Visual image of my abbreviation for 'reproductive tendencies,' then of some eggs, then of two sheets of typewritten matter with a carbon sheet between. Verbal image 'manifold.'

Perhaps this is the least inappropriate place to say a few words about a series of experiments which do not belong exclusively to the discussion of any particular attitude, and yet throw some light upon the whole subject. Their only connection with the preceding is the fact that they were performed by the same observer (MC) at about the same time; their bearing is rather on imageless thought than on the attitudes proper. It will be remembered that Marbe used the *Ausfragemethode*, and that his questions were criticised upon the ground that they were too simple, and could be answered merely associatively, without any thought. Bühler avoided this danger by giving aphorisms, which offered some difficulty to understanding, or asking questions, which required, for instance, the consideration of whole periods in the history of philosophy in a brief time.

The object of the present series was to combine these methods into a differential or contrast method in order to bring out the difference in consciousness between the answer which required thought and that which did not. The subject chosen was history, because this was of special interest to the obser-

ver. She was asked 50 questions, chosen indiscriminately from many periods of general history, and ranging in difficulty from "When was America discovered?" to "What were the constitutional difficulties in the way of reconstruction after the Civil War?"

We find that visual images play an important part in this observer's memory of history. Centuries and special dates within them are seen schematically and often in colors, while persons and events are assigned to certain periods because they are seen to be in them or like them in color. *Name the first five presidents of the United States.* Answer: *Washington, J. Adams, Jefferson, Madison, Monroe.* Visual image of each man as his name was given. Jefferson was especially clear. With Adams, image of date 1801. Schematic image of length of terms. It was dark colored and had some spaces marked off, twice as much for two terms as for one. In connection with Madison, image of War of 1812 and of an old book on the subject. Adams seemed to stand in a corner, a turn between two centuries.

The facts of history as well as their dates are in many cases apparently read off from visual images which had represented them in the past. *Was there any connection between the French and American Revolutions?* Answer: *Yes. The French assisted the American Revolution and are said to have incited it. The French Revolution was influenced by the example of the American.* Visual image of a book on the subject. Image of the American Revolution and of arms and ammunition on a ship being sent by the French to America. Then of a Frenchman sitting in a coffee-house finding out public opinion about the Revolution. I seemed to see the two wars at the same time, and to know that the American happened first and was an example. *What became of the Celts when the Teutons invaded England?* Answer: *They were partly absorbed, partly exterminated, partly pushed into Wales.* Image of Wales and of Celts and Teutons fighting. Then of the whole country with some Celts and Teutons intermingled. This is what made me say 'absorbed.'

Sometimes the answer is so familiar and comes so readily that there is very little else in consciousness. *When was America discovered?* Answer: 1492. Visual image of the date with a red halo, also of Columbus in Spain with distance and direction to Spain. *Who first sailed around the globe?* Answer: *Magellan.* Visual image of Straits of Magellan, of the man, and later of the name. *When was the fall of Rome?* Answer: 476. Image of date, and of Rome surrounded by barbarous hordes.

Let us summarise the facts which the two hundred and seventy introspections show in regard to the understanding of words and sentences.

(1) A word which is not familiar calls up others which are similar to it in sound, and the images are appropriate to these familiar words. (2) Words like 'to,' 'which,' 'of,' which do not ordinarily occur without context and do not refer to an object that can be represented by an image, also show to some extent the tendency toward mere auditory association. Their appearance in this strangely unconnected position is usually followed by groping or blankness of consciousness. They often form a context for themselves, by verbal association with some word which could grammatically follow them. (3) Words which refer to objects of sense, visual or otherwise, are often represented by images. (4) Words which are capable of more

than one interpretation usually excite in quick succession images appropriate to the different meanings. (5) In comparing the average length of the introspections on words, sentences, and paragraphs, we find that they are in no sense proportional to the length of the stimulus; on the contrary, they are all of about the same length.

The very fact that the single words stood alone, out of all connection, introduced an unfamiliar element, and gave opportunity for all sorts of associations. Experiments upon single words are, in fact, comparable to 'free associations.' When a word comes without context, and the time which the introspection is to cover is not definitely limited, the images, visual, verbal and other, which are aroused, are likely to be numerous and varied. Most of them refer in some way to some meaning of the word, or to its connection with events of our own lives; but some may be irrelevant. If now, we read the same word as part of a short sentence, we get somewhat the same effect as when an association is guided by a word just heard, or when the observer adds to the *Aufgabe* an additional self-imposed condition. The *Aufgabe* now is not "Get any meaning of the word," but "Get a meaning which goes with these other words." Moreover, the time is shortened; the words follow one another in quick succession. This limitation in time tends to inhibit part of the images, and the context determines which of them shall be suppressed.

It may be objected that it is not always the wrong association that is inhibited. Our introspections, as well as those of previous writers, show that often the images are inadequate, irrelevant, or even contradictory. So far as inadequacy is concerned, however, we have no criterion, save the facts themselves, by which we can decide how clear or complete an image must be in order to carry a meaning. Again, the image which is logically contradictory may yet have enough in common with the meaning of the word to be psychologically adequate to this meaning. Two words cannot, indeed, be spoken of as contradictory, unless they have something in common; they must at least belong to the same universe of discourse, to the same context; and it is just this context which, recalled in any form whatever, constitutes a more or less general meaning. Logically, it is not easy to see why a bird, described as white with a black ring around its neck, should be imaged in its ordinary colors; or why a description of dogs carried in a basket should give rise to images of cats jumping out of a panier. Our own experiments are not free from such anomalies. The word 'noiseless' arouses an image of some particular noisy children; 'electric' is followed by an image of a park which is not electric; 'home' recalls France,

and the idea that the language of that country has no word of the same meaning. In every one of these cases, however, there is sufficient connection between the logical meaning of the word, and the psychological content of the act of understanding, for the latter to carry a general meaning.

The third case, that in which the imagery is neither inadequate nor contradictory, but irrelevant, is less easy to explain. It is a fact of observation that the wrong meaning is not always inhibited by the setting and the additional *Aufgabe*, but runs along parallel with the understanding of the sentence. Every word, however, is not of equal importance for the understanding of the whole; and even if a single word is given a wrong interpretation at the time of reading, the meaning of the whole may be fairly clear. The introspections show cases in which the word was seen only in part, or was at first misread, and the context of the wrong reading immediately appeared in consciousness. This often occurs in everyday experience, without attracting attention. The mistake is corrected as we go on, and the wrong image is replaced by others which are more consistent with the meaning of the situation.

These attempts at explanation are tentative only. We have the fact that understanding may at times be mediated, psychologically, by images which logically are inadequate, irrelevant or directly unsuited to their office. The road to final explanation lies through a detailed study of the conditions under which such representations of the act of understanding take shape. Their appearance in experiments like our own probably depends, in many cases, upon ingrained habits of reproductive tendency, which by lapse of time are inaccessible to introspection. Our aim must be to catch them in the making,—either by casual observation in everyday life, or by way of specially shaped observations in the laboratory.

Unanalysable feelings of RELATION have been postulated by various writers. Woodworth, in particular, has made experiments with words and with papers of different colors and shapes, arranged according to the rule of three, which, as he believes, show that a relation is sometimes conscious as an 'imageless' thought. His verbal stimuli were presented in the form London: England : : Paris: *X*. The observer was to supply the fourth term of the proportion, and to give a complete introspection. The reports fall into four classes. (1) When the relation is easy to grasp and the missing term is readily found, very little consciousness appears. "There was nothing in my mind," said one of the subjects, "except that I wanted to answer your question right." The answer

comes immediately, on the hearing of the three given terms. (2) When there is more difficulty, the relation sometimes receives a name before the answer is found. (3) Sometimes the relation is pictured, in some form of imagery. (4) Sometimes the subject reports that he felt the relation, but did not name it or have an image of it.

Unfortunately, the author does not tell us what fraction of the whole number of experiments belongs to the fourth class. If the reports of this group were numerous, they might offer some evidence for the existence of unanalysable feelings of relation. If they were not, they may well be explained by the incompleteness of introspection. All observers probably fail at times to analyse into simplest terms, or to report all the contents of consciousness.

We have ourselves made several series of experiments on this subject, all of which, in form at least, are based on those of Woodworth. In the first series, the stimulus consisted of three words in the form of a proportion with the fourth to be supplied, exactly as in Woodworth's experiments. In fact, a few of the examples were taken bodily from his paper. The observers were V, G, and F.

The relation is sometimes present in consciousness as a word.

V. *A book: a magazine: : a chair: a stool.* Visual image of a red book and a magazine side by side. At hearing the word 'chair' great surprise,—a muscular contraction and a gasp. I looked at a chair in the room, then at a table. Tendency to say 'table,' because I had a kinæsthetic idea that a book is squarer and higher than a magazine. Same of table and chair. This was inhibited, I don't know how. Visual image of a footstool. Then I said 'less than a chair.' Then said 'stool' aloud. No balance. The whole took effort.

F. *Red: blue:: green: yellow.* I started to say this automatically. Then I repeated the stimulus and said 'intermediate' verbally. Some kind of consciousness that meant 'principal colors'. I did not say 'principal.'

G. *Family: individual: tree: fruit.* Verbal completion in background. Said 'group to one.' When you said 'tree,' I said 'tree is individual itself.' This was in the background. Articulated 'Tree produces what? Fruit.' There was no association ready, and I had to make one by making a new sentence. 'Species' also present verbally. Verbal part in background.

Sometimes the relation is represented visually, as in Woodworth's third division.

F. *Book: chair :: table: floor.* I saw a chair with a book on it. Visual image of a table on the floor. The relation was kinæsthetic and visual.

V. *Man: boy :: woman: girl.* Visual image of a small boy in a blue sailor suit. Meaning of 'woman' carried by a vague image of a red plaid skirt. Then a blank. I said 'girl,' but I don't know why. I was surprised when I said it. This was vaguely organic, and a little gasp occurred. The relation between man and boy was one of height,—a tall and a short line side by side. Just after I finished, I thought perhaps I should have said 'little girl.' This was vaguely verbal.

By far the larger number of relations which were carried in sensory terms could not be put strictly in either of these

classes, but were combinations of images from different sense departments. Organic sensations were prominent for all the observers.

V. *London: England :: Paris: France.* I did not think of London till I heard 'England.' Vague image of the map of England with a black dot standing for London. Kinæsthetic image of drawing a circle and putting a dot inside it. At 'Paris' I had a kinæsthetic image of making a dot, and visual image of a black dot. Without any effort I said 'France,' and had image of drawing a circle around the dot. Very pleasant. The pleasantness included a kind of balance which was vaguely visual and organic. At 'London' image of a capital L, and at 'France' image of a capital F.

F. *To: fro :: back: front.* Repeated the stimulus twice. Organic and kinæsthetic images or sensations of swinging arm in a circle while saying 'to and fro.' I marked the rhythm with words and breathing. Very vague visual images. A thin black thing which was moving like a pendulum. I could not see the whole pendulum, but only the arc that it described. Organic sensations with the pendulum. Suddenly 'back' coincided with one swing, and 'front' with the return swing. The visual part was six or eight feet off and below me.

G. *I: we :: he: they.* This was kinæsthetic. I put 'I' in the first line, 'we' in the fourth, he (he, she, it) in the third, and 'they' in the sixth. This was the declension in an old grammar. I did not see 'he, she, it,' but the line was long kinæsthetically, while 'I' and 'they' were dots.

Woodworth's fourth class, of cases in which the relation was present in consciousness but not analysable into sensory or affective terms,—the class upon which he bases his whole conclusion,—reduces, in our own experiments, to two equivocal instances.

G. *Color: brightness :: tone: intensity.* (G thought of intensity as an attribute.) Short period of confusion, which was muscular contraction. I repeated verbal stimulus and completed it almost automatically. Background filled with vague memories, in visual images and eye-movement, of experiments and discussions on brightness and intensity.

F. *London: England :: Paris: France.* I fell into the swing as soon as you started to read. It was familiar. This was a real change of muscular attitude, a sort of relaxation.

Woodworth's first class, in which the relation is not present in consciousness in any form, is abundantly illustrated.

G. *Father: son : : mother: daughter.* Purely verbal. The vaguest articulation of 'mother.' No relation about it.

G. *Red: blue : : green: yellow.* No relation. I was listening to the colors, and added the one you did not name, as I should have been ready to mention any one.

F. *Is: are :: was: been.* Verbal rhythm. I used to say 'is, are, was, been.' There was just the swing.

F. *Boy: man :: girl: woman.* Verbal. No image.

V. *Is: are :: was : (am) were.* Strong tendency to say 'am,' though I knew it was n't right. This was kinæsthetic. When the stimulus was repeated, the 'r' in 'are' caused me to say 'were,' the two r's balanced. I did not think of the meaning till afterwards.

These seemed to be cases of mere association, in which the relation had no part. In order to test this conclusion, the method was slightly modified in two ways. Mingled with the

other stimuli were proportions made of pairs of familiar expressions, in which there was either no relation or one that could be recognised only with some ingenuity. The question implied was, whether these pairs would be replied to as promptly as the true relation pairs. Almost invariably this was the case. The other modification was the introduction of proportions in which the relation was reversed, that is, in which the third term really corresponded with the second and the fourth (to be supplied) with the first. In a few cases the observer refused to react; but this inhibition occurred only after such a proportion had already been given, and automatically reacted to. After the reaction, the observer sometimes saw that the order was wrong, and was therefore more careful the next time. In some instances, the change was never discovered at all. The following will illustrate the reactions to familiar phrases.

V. *Up: down :: out: in.* I responded immediately without reasoning, then wondered aloud why this was right. Felt tired.

G. *Loud: soft :: dark: light.* Almost reflexly. A little pause of hesitation during which I quickly went over the whole again in abbreviated form. No relation conscious.

F. *Live: die :: sink: swim.* Mere verbal association. When I stop to think, there is opposition, but this was not conscious. (F. can repeat the quotation from which this proportion is taken, but was not conscious of it when the reaction was made.)

Some of the wrong proportions were reacted to as follows:

F. *Father: mother :: aunt: uncle.* Quite verbal. I don't know whether the answer is right or not.

F. *Day: night :: winter: summer.* It is a muscular attitude which makes me answer. I was set for the rhythm, and reacted just as I should fill out an incomplete line of metre. The word came up of itself; winter and summer go together.

V. *Day: night :: winter: summer.* I said 'summer' because it seemed to belong there. I heard myself saying it before I said it. Later had a kinæsthetic feeling that it was backwards,—lack of balance and a feeling of twisting around.

A few experiments were performed, with the same observers, by means of little slips of colored paper. Three colors were given, and a fourth was to be added that would have the same relation to the third as the first to the second. The relations were not complicated by the introduction of different shapes and sizes. The method was not promising, and was soon discarded. When three of the four principal colors of the spectrum were given, the fourth was added without any 'feeling of relation.' It was simply the 'filling out of the series.' In the remaining cases, the relation was represented visually, verbally, or kinæsthetically.

The next series was intended to approach the problem of relation from a genetic standpoint. The purpose was to

establish some arbitrary relation, and to observe what took place in consciousness as it became more and more familiar.

The observer was told that
S is the cause of T
H" " " " K
L " " " " M

The proportions then combined three of the letters, in various ways, and the observer was asked to add the fourth term. The association was made by G originally from the written page. He was given the above statement to read and fix in mind. He disregarded the word 'cause' entirely, and remembered the letters as related merely by spatial arrangement, not visually, but by eye-movement. A movement of the eyes across and down must be followed by another in the same direction in order to make the relation correct. When the proportion H: M:: S:? was given, G reacted with K because this reply made two parallel diagonal lines. Although the proportions were too easy at the start to give the method a fair trial, the 76 experiments done with G show some effect of habit. At the beginning, the whole relation was carried by eye-movement; it was movements that were equated and that therefore represented the relation. Verbal ideas sometimes entered in. As early as the fourth experiment, H: K :: L: M, G reports: 'I did not jump to L. I went down from K to M.' Abbreviation is beginning. From the twenty-eighth, the verbal reactions are numerous, though the eye-movement continues to some extent to the end. When H: K :: L:? was given, G simply went down the alphabet automatically.

In order to prevent this unforeseen possibility, different letters were given to F.

D is the cause of E
R " " " " L
T " " " " Q

The statement was read to him, in order that a merely spatial relation might not fix itself in the mind. Nevertheless, this result appeared, to a certain extent; the causal idea was entirely forgotten. Two letters of a pair were associated, and at first F did not distinguish the first from the second. If both numbers of one pair were given and only one of another, the missing term was immediately added. F also tended to read meanings into the letters. These methods of association made the reaction so easy that it was automatic, and conscious content was lacking from the first. F explained his reactions by saying that they 'just go together.' As the series offered no chance for improvement, it was abandoned after the nineteenth experiment.

The same letters were read to V. She visualised the top pair with 'cause' written between them, and the others below with ditto marks under the 'cause,' though she had not seen the paper. Sometimes the reactions were merely read off from this image. The fourth term was often supplied because it went with the third; there was no reference to what had come before. Only fifteen tests were given.

In all the experiments by this method, there is not a single case of a relation being consciously carried in non-sensory terms. Either it is definitely describable, or it is not conscious at all and the reaction is automatic. In the latter case it was usually immaterial to the observer whether the answer was right or not. Sometimes it was worked out carefully afterwards, and judged as to correctness; but in that event the relation was represented in some form of imagery.

As this method had proved too easy to exhibit the automatic reaction in process of development, another was devised. This time the stimuli belonged to three sense-orders, auditory, tactual and kinæsthetic. The observer sat with his right arm on a Sanford elbow-board, and the index-finger of his left hand on a small lever which moved up and down. The tactual stimuli, large and small pieces of sand-paper and of felt, were presented by being laid under the fingers of the right hand on the arm rest. A low and a high tuning-fork stood near by. The observer was told that the low tone was to be thought of as large, the high as small, while each one might be either strong or weak. The sand-paper was intended to represent strength or harshness, the felt weakness or softness; each might be either large and small. The arm movement was large, the finger movement small, while each might be either strong or weak. This arrangement was, no doubt, arbitrary, but it was arbitrary for a purpose. Easily perceived relationships had proved inadequate, and it was hoped that the artificiality of these new ones would make the reactions sufficiently difficult. Only G and V took part in this series of experiments.

G's reactions were at first almost without exception mediated by verbal expressions of the relation. He would say 'loud-soft' or 'strong-weak,' and the reaction followed. The verbal ideas were sometimes more complex than this, or the relation was partly kinæsthetic. It will give some idea of the progress of mechanisation if we fractionate the results. In the first 24 experiments there is not a single automatic reaction. The relations are carried in verbal terms. From the twenty-fifth to the forty-eighth there are 11 automatic reactions, in which the relation was not conscious. From the forty-ninth to the seventy-second there are 13, and from the seventy-third to the ninety-sixth, 17. The reactions are most often automatic when the three given stimuli are from the same sense department. There were only four possible variations in one sense department, and when three were given the fourth followed automatically. The series was carried so far, however, that even proportions between stimuli of different sense orders were sometimes reacted to automatically.

V made 40 experiments by this method but with no new result. The relation was usually verbal, once or twice kinæsthetic, and several times purely associative.

When we consider all of the relation experiments, we see that by far the larger part of the reactions were accompanied by some 'consciousness of relation' in terms of sensory or verbal images, and that the rest prove to be mere associations or else tendencies to fill out a group, by adding the inevitable fourth member, without any consciousness. Woodworth's fourth group is not paralleled in our experiments.

Conclusion

In conclusion, we may attempt to sum up the arguments which make against the simplicity of the 'conscious attitude'

and the existence of 'imageless' thought. These may be divided into the negative or critical, and the positive or those based upon our own experimental work. Under the former head the following may be noted:

(1) Having been named and negatively defined by Marbe, the *Bewusstseinslagen* are henceforth taken for granted. They are reported, along with sensation and image and feeling, in the analyses of complex states, and little or no attempt is made to analyse them.

(2) Nevertheless, they are, on several occasions, at least partly analysed, as witness Orth's account of doubt, Messer's and Watt's of trying to remember, and the discussions of the *Aufgabe*, which show it to be an attitude derived by practice from an analysable situation.

(3) The cases in which thought-elements or imageless thoughts or attitudes are reported as the 'consciousness that,' etc., are cases not of psychological description, but of the translation into words of the meaning of a conscious state (*Kundgabe*).

(4) Our own conclusions are based upon the introspections of seven observers, of whom all but one had had several years of psychological training. These observers were not all of one type, but ranged from the strongly visual to one who almost never has visual images, and from those who almost never report kinæsthetic sensations to those for whom these sensations and images are essential. These seven persons wrote, altogether, somewhat more than fourteen hundred introspections. In the series with point-letters alone, over four hundred cases of attitude are specifically reported,—aside from recognition, which is assumed to be present in all observations. Of these four hundred, about one fourth were merely named, while the remaining three fourths are more or less completely analysed. When the attitudes occur often enough to give a basis for generalisation, there is striking agreement between different observers and for the same observer at different times, and we are thus able to pick out, with a fair degree of assurance, the pattern of consciousness which represents a given situation. The introspections of any one observer show different stages of clearness and intensity of imagery, which allow us to connect, by a graded series of intermediate steps, a complex of vivid and explicit imagery with a vague and condensed consciousness which we suppose to represent what is called 'imageless' thought. The *Aufgabe*, recognition, and the feeling of relation are shown to be capable of development, by a process of change and mechanisation, from states which are obviously complex and imaginal.

It may, we think, be fairly said that the attitudes here analysed are typical of the whole class; they are certainly among those most often mentioned by writers on the subject. But, if part of the class can be reduced to simpler terms so often, so definitely, and so uniformly, there is every reason to believe that the rest will show themselves similarly complex, when they are subjected to the same analytical treatment. The general conclusion to be drawn from the sum of our results is that conscious attitudes can be analysed into sensations and images and feelings, or traced genetically to such analysable complexes, and therefore do not warrant the proposal of an additional conscious element.

AN EXPERIMENTAL DEMONSTRATION OF THE BINAURAL RATIO AS A FACTOR IN AUDITORY LOCALIZATION

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I. HISTORICAL

It is difficult to ascertain just when the belief in the binaural ratio as a factor in auditory localization came into vogue. The experimental arguments offered in favor of this belief, however, are not so hard to trace. They began with the tuning-fork experiment of Weber,[3] and have been continued

[1] *Vide* von Kries: *Ueber das Erkennen der Schallrichtung*, Zeitschr. f. Psychol. u. Physiol., I, 1890, 236-251; and Dunlap: *The Localization of Sounds*, Psychol. Rev., Monog. Suppl., Vol. X, No. 1, 1908, pp. 5, 8, 15.

[2] *Vide* Dunlap: *Op. cit.*, pp. 5, 10, and 15.

[3] Weber: Programm. Coll., 4 2. This experiment was not offered by Weber as an argument for the binaural ratio of intensity, although it has frequently been cited as furnishing such argument.

by the work of Fechner,[1] Rayleigh,[2] Politzer,[3] von Kries and Auerbach,[4] Tarchanoff,[5] Steinhauser,[6] Urbantschitsch,[7] Thompson,[8] Kessel[9] von Bezold,[10] Schaefer[11], Smith,[12] Bloch,[13] Pierce,[14] Matsumoto,[15] Melati,[16] Stenger,[17] Starch,[18] and Wilson and Myers.[19]

A résumé of this work down to 1901 has been given by Pierce It will be sufficient, therefore, for the purpose of this report to continue the résumé up to the present time, and to make a brief statement of all the lines of argument that have been advanced for the binaural ratio as a factor in auditory localization.

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[1]Fechner, G. T.: *Ueber einige Verhältnisse des binocularen Sehens* (Chap XVIII, *Ueber einige Verhältnisse des zweiseitigen Hörens*). Abhdlg. d Sächs. Gesellsch. d. Wiss. (Mathemat. Klasse V), Bd. V, S. 543. 1861.

[2]Rayleigh, Lord: *Our Perception of the Direction of a Source of Sound*, Trans. Mus. Ass. 1876; *Acoustical Observations*, Philos. Mag. (5) Vol. III, 1877, p. 456.

[3]Politzer: *Studien über die Paracusis Loci*, Archiv. f. Ohrenheilk. 1876, XI, 231.

[4]von Kries u. Auerbach: *Die Zeitdauer einfachster psychischer Vorgänge*, Archiv für Anatom. u. Physiol., 1877, 321-337.

[5]Tarchanoff: *Das Telephon als Anzeiger der Nerven und Muskelströme beim Menschen und den Thieren*, St. Petersburger med. Wochenschrift, 1878, No. 43, pp. 353-354.

[6]Steinhauser, Anton: *The Theory of Binaural Audition: A Contribution to the Theory of Sound*, Philos. Mag., Ser. 5, Vol. VII, 1879, pp. 261-274.

[7]Urbantschitsch, V.: *Zur Lehre von der Schallempfindung*, Pflüger's Archiv, XXIV, 1881, 579.

[8]Sylvanus Thompson: *The Pseudophone*, Philos. Mag. (5), VIII, 1879, 385-390. *On the Function of the Two Ears in the Perception of Space*, Philos. Mag. (5), XIII, 1882, 406-416.

[9]Kessel: *Ueber die Functionl der Ohrmuschel, bei den Raumwahrnehmungen*, Archiv f. Ohrenheilk. XVIII, 1882, p. 120.

[10]W. von Bezold: *Urteilstäuschung nach Beseitigung einseitiger Harthörigkeit*, Zeitschr. f. Psychol. u. Physiol., 1890, pp. 486-488.

[11]Schaefer, K. L.: *Lokalisation diotischen Wahrnehmungen*, Zeitschr. f. Psychol. u. Physiol., I, 1890, S. 300-309.

[12]Smith, G.: *How do we Detect the Direction from which Sound Comes?* Cincin. Lancet-Clinic, n. s., XXVIII. 1892, p. 542.

[13]Bloch: *Das binaurale Hören*, Wiesbaden, 1893, pp. 61; Zeitschr. f. Ohrenheilk., XXIV, 1893, pp. 25-86.

[14]Pierce, A. H.: *Studies in Space Perception*, 1901.

[15]Matsumoto: *Researches in Acoustic Space*, Studies from the Yale Psychological Laboratory, V, 1897.

[16]Melati, Gino: *Ueber binaurales Hören*, Philos. Studien, XVII (3), 1901, 431-461.

[17]Stenger: *Zur Theorie des binauralen Hörens*, Zeitschr. f. Ohrenheilk., XLVIII, 219.

[18]Starch, D.: *Perimetry of the Localization of Sound*, Psychol. Rev., Monog. Suppl. (Univ. of Iowa Studies), Vol. IV, No. 28, 1905, pp. 1-45; *ibid.*, Vol. IX, No. 2, 1908, pp. 1-55

[19]Wilson, H. A. and Myers, C. S.: *The Influence of Binaural Phase Differences in the Localizations of Sound*, The British Journal of Psychology, 1908, II, pp. 362-386.

Since 1901 reports of work on the general subject of auditory localization have been published by the following investigators: Lobsien,[1] Angell and Fite,[2] Melati,[3] Gamble,[4] Angell,[5] Seashore,[6] Bing,[7] Urbantschitsch,[8] Stenger,[9] Bard,[10] Starch,[11] Rayleigh,[12] More and Fry,[13] Bowlker,[14] Wilson and Myers,[15] and Hicks and Washburn.[16]

Of these only six bear with sufficient directness and definiteness upon the subject of this report to warrant consideration here; namely, the papers of Angell, Angell and Fite, Starch, Rayleigh, More and Fry, and Wilson and Myers.

In 1903 Angell,[17] in furtherance of the suggestions and ob-

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[1]Lobsien, Marx: *Ueber binaurales Hören und auffällige Schalllocalisation.* Zeitschr. f. Psychol. u. Physiol., XXIV, 1900, S. 285-295.

[2]Angell, J. R. and Fite, W: *The Monaural Localization of Sound,* Psychol. Rev., VIII, 1901, pp. 225-247; *Further Observations on the Monaural Localization of Sound, ibid.,* 449-459.

[3]Melati, Gino: *Op. cit.*

[4]Gamble, E. A. McC.: *The Perception of Sound Direction as a Conscious Process,* Psychol. Rev., IX, 1902, 357-373; *Intensity as a Criterion in Estimating the Distance of Sounds,* Psychol. Rev., XVI, 1909, 416-426.

[5]Angell, J. R.: *A Preliminary Study of the Significance of Partial Tones in the Localization of Sound,* Psychol. Rev., X, 1903, pp. 1-15.

[6]Seashore, C. E.: *Localization of Sound in the Median Plane,* Univ. of Iowa Studies in Psychology, 1899, 11, 46-54; *A Sound Perimeter,* Psychol. Rev., X, 1903, pp. 64-68; *The Localization of Sound,* Middletonian, 1903 (Dec.), pp. 15.

[7]Bing, A.: *Bemerkungen zur Lokalisation der Tonwahrnehmung,* Monatschr. f. Ohrenheilk., XXXVIII, 1904, 220-225.

[8]Urbantschitsch, V.: *Ueber die Lokalisation der Tonempfindungen,* Archiv. f. d. ges. Physiol. (Pflüger's), CI, 1904, 154-182.

[9]Stenger: *Op. cit.*

[10]Bard, L.: *L'orientation auditive angulaire,* Archiv. gen. de Med., CXCV, 1905, 257.

[11]Starch, D.: *Op. cit.*

[12]Rayleigh, Lord: *On our Perception of Sound Direction,* Philos. Mag., XIII, Ser. 6, 1907, pp. 214-232; *Acoustical Notes, Sensations of Right and Left from a Revolving Magnet and Telephones, ibid.,* pp. 316-319; *Acoustical Notes, Discrimination between Sounds Directly in Front and Directly Behind the Observer, ibid.,* XVI, 1908, pp. 240-241.

[13]More, L. T. and Fry, H. S.: *On the Appreciation of Phase of Sound Waves,* Philos. Mag., Ser. 6, XVII, 1907, pp. 452-459.

[14]Bowlker, T. J.: *On the Factors Serving to Determine the Direction of Sound,* Philos. Mag., Ser. 6, XV, 1908, pp. 318-332.

[15]Wilson, H. A. and Myers, C. S.: *The Influence of Binaural Phase Differences on the Localization of Sounds,* British Journal of Psychology, II, 1908, pp. 362-384.

[16]Hicks, J. and Washburn, M. F.: *A Suggestion towards a Study of the Perception of Sound Movement,* Am. Jour. of Psychol., XIX, 1908, 247-248.

[17]Angell, J. R.: *Op. cit.*

servations made by Rayleigh,[1] Thompson,[2] Mach,[3] and Pierce,[4] undertook a systematic investigation of the influence of timbre on the localization of sound. Careful observations, in the open air, were made of the accuracy of the localization of simple tones and of clangs. The sounds employed were a tuning-fork of 1,000 vibrations per second, a stopped pipe of 768 vibrations, a reed pipe of 768 vibrations, a bell with a fundamental tone of 2,048 vibrations, and the noise made by a telegraph sounder. An interpretation of his results, based on the relative accuracy of localization at different points in the vertical, horizontal, and transverse planes, led him to conclude that intensity differences alone are sufficient to enable our confident and correct assignment of the sound (even in case of pure tones) to the median plane, the lateral hemisphere, and, in a general way, to the transverse plane. But accuracy as regards altitude in the transverse plane, or in the region between the transverse plane and the median plane, is apparently dependent upon the modifications of timbre which complex sounds, coming from different directions, undergo, through changes in the intensity of their partials. Considered with reference to its bearing on the binaural ratio, the paper, in its general tone, is against the ascription of too much importance to this ratio as a factor in localization. This position is further supported by experiments conducted by Angell and Fite.[5]

The object of these experiments was to determine the localizing power of subjects who were deaf in one ear. In the first series, only one subject was experimented upon; in the second, several were used differing in age and varying in the length of their period of deafness from one to thirty years. The results of the experiments are as follows. (1) These subjects, especially when practiced, are not greatly inferior, in their power to localize, to subjects of normal hearing.[6] Dis-

[1]Rayleigh, Lord: Transactions of the Musical Association, 1876; and Philos. Mag. (5), III, 1877, p. 456.

[2]Philos. Mag., XIII, 1882, p. 415; *ibid.* (5), VIII, 1879, pp. 385-390.

[3]Mach, E.: *Bemerkungen über die Function der Ohrmuschel*, Archiv f. Ohrenheilkunde, IX, 1875, p. 72; *Bemerkungen über den Raumsinn des Ohres*, Poggen. Annalen, CXXVI, 1865, p. 331; *Ueber einige der physiologischen Akustik angehorigen Erscheinungen*, Sitzungsberichte der Wiener Akademie, Abth. 2, L., 1864. pp. 342-363; *Zur Theorie des Gehororgans*, *ibid.*, Abth. 2, XLVIII, 1863, pp. 283-300.

[4]*Op. cit.*, pp. 92 and 163.

[5]Angell, J. R. and Fite, W.: *Op. cit.*

[6]It is assumed here that these writers would exercise caution in drawing conclusions, with regard to the relative importance of timbre and the binaural ratio of intensity in normal subjects, from the localizing power shown by subjects who have been deaf in one ear for a number of years; because the latter, deprived of the use of the binaural ratio as an aid to localization, would doubtless develop a discrimination of direction based upon difference

tinctions between front and back may be even sharper for these subjects than for those of normal hearing. The localization, however, is generally not so prompt for them as for the normal subject, nor are these subjects so accurate in dealing with unfamiliar sounds. (2) Complex sounds, especially those in which qualitative differences can be introspectively distinguished for the different positions, are localized best. The more nearly the sound approximates a simple tone, the more inaccurate is the localization. "Genuinely pure tones are essentially unlocalizable." (3) There is a marked increase in accuracy with practice. The accuracy of the practiced monaural subject, for example, was found to compare very favorably with that of the unpracticed normal subject. (4) Accuracy was also observed to sustain a close relation to the length of time the defect had existed, and to the age at which it began. For example, subjects of advanced age who had recently become deaf showed much poorer ability to localize than younger subjects who had been deaf for a number of years.

Working in 1905 and again in 1908, Starch[1] carried out an extended series of experiments on the localization of simple tones and clangs. Both monaural and binaural hearing were investigated. In the experiments with clangs, a singing flame, a Galton whistle of 10,000, 20,000, and 30,000 vibrations, the human voice, an electric hammer, a wooden clapper, and a whiff of air were used as the sources of sound. In the experiments with simple tones, a tuning-fork of 100 vibrations per second was used. In the latter experiments, tests were made at different points in the different planes of direction, (a) of the accuracy of localization, (b) of the size of the j. n. d. of direction, (c) of the limen and j. n. d. of intensity, and (d) of the j. n. d. of pitch. A number of conclusions were drawn relative to intensity and timbre as factors in localization.

Space will be taken here only for a résumé of the evidence bearing upon the binaural ratio as a factor in normal hearing, and upon intensity difference as a factor in monaural hearing. No new evidence is advanced in support of the binaural ratio, the object of the experiments apparently being a testing of the arguments already advanced by Rayleigh, Thompson,

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in timbre, considerably beyond that possessed by the normal subject. This supposition is, in fact, borne out by their own results, which show how poorly subjects recently deaf localize as compared with those in whom the defect had existed for a number of years. For example, Case F. (*Op. cit.*, p. 453), aged 60 years, deaf one year, gave correct judgments of location in only 19.5 per cent. of the total number of cases; while Case C., deaf from 26 to 30 years, gave 55 per cent. of the total number correctly.

[1]Starch, D.: *Op. cit.*

Bloch, and others. Starch finds these arguments confirmed by his own results. The arguments are: (a) the presence of front-back confusion, and its special case, the difficulty of median plane localizations; (b) the inferiority of monaural localization; and (c) the occurrence of the greatest accuracy of localization at points where slight changes in the binaural ratio are most readily perceived, *i. e.*, in front and back near the median plane, and the poorest where these changes are least readily perceived, *i. e.*, at the sides near the aural axis. Starch disagrees with Angell as to the factors in monaural localization. He maintains that, in addition to changes in the quality of a sound when it comes from different directions, there are systematic changes in intensity, which serve as a localizing clue.[1] The following evidence is given for systematic changes in intensity: (1) the limen for intensity, which is lowest in the region of the aural axis, and highest in front and back; (2) the observers' introspections with supraliminal sounds; (3) the distance tests, which showed that a sound is estimated to be nearest in the region of the aural axis. That these changes of intensive serve as a localizing clue is attested (1) by the introspection of the observers, and (2) by the poor localization when the intensity of the stimulus was varied frequently during the course of a series of experiments. The smaller j. n. d. of direction for front and back, as compared with the region near the aural axis,[2] he thinks, however, cannot be due to the intensity factor, for there is no corresponding difference in the intensity j. n. d.'s in these positions. He seems inclined to attribute this smaller j. n. d. of direction in front and back, at least in the case of his own experiments with the tuning-fork, to the qualitative factor; for his results show a smaller j. n. d. for pitch in front and back than in the region of the aural axis. Starch interprets his results as, on the whole, favoring the intensity theory. The traditional intensity theory is in the main correct; but, in order to account for monaural localization, and localization in the median plane and the planes parallel to it, this theory must be supplemented by the quality and the monaural intensity factors.

In February, 1907, Rayleigh[3] published an article in which he attempted to show that the binaural ratio cannot be a factor in the localization of sounds with a vibration fre-

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[1]Starch: Psychol. Rev., Monog. Suppl., No. IV, Vol. VI, 1905, pp. 11-12; *ibid.*, No. V, Vol. IX, 1908, pp. 52-53.

[2]*Vide* Bloch: *Op. cit.*, p. 55-58; Matsumoto: *Op. cit.*, p. 65-69.

[3]Rayleigh, Lord: *On our Perception of Sound Direction*, Philos. Mag., Ser. 6, XIII, 1907, pp. 214-232.

quency of 128 per second, or less. In a previous article, published in 1876,[1] he had shown by calculations relating to the incidence of plane waves upon a rigid spherical obstacle, that a sound-wave of that vibration-frequency travelling in the line of the aural axis could not differ in intensity at the orifices of the two ears by as much as one per cent. of its total intensity. It is difficult for him to see how so small a difference could play a very important part in localization; yet he finds, at least within the limitations of his somewhat rough tests, that the tones of forks of 128 and 96 vibrations per second are localized as accurately as those of higher frequency. He infers, therefore, that there must be some other localizing clue for tones of low pitch. The only alternative to the intensity factor, he thinks, is a direct recognition of phase differences by the auditory organ.[2]

He discusses phase difference in its relation to localization as follows. When the stimulus is at one side, in the line of the aural axis, the opposite ear is "roughly about one foot" (measured on the circumference of the head) farther from the stimulus than the nearer ear. For a fork of 128 vibrations per second, this would make the phase difference between the ears about ⅛ period; for a fork of 256 vibrations, about ¼ period; for a fork of 512 vibrations, about ½ period; and for a fork of 1,024 vibrations, about a whole period. "Now it is certain," he says, "that a phase relationship of ½ period furnishes no material for a decision that the source of sound is on the right rather than on the left, seeing that there is no difference between a retardation and an acceleration of ½ period. It is even more evident that a retardation of a whole period or any number of whole periods would be of no avail."

Having shown that sounds of 128 vibrations or less per second reach the ears in a difference of phase which *a priori* might be considered recognizable in sensation, Rayleigh next attempts to show that these differences actually furnish the clue for the localization of the graver tones. He works with two slowly beating tones of near 128 vibrations per second. In completing a cycle or beat, the phase differences of these tones assume all possible values. When the tones are led to the two ears simultaneously, but separately, he finds that instead of getting plainly recognizable beats, as would have occurred had both the sound-waves been given to each ear, the

[1]Rayleigh, Lord: *Our Perception of the Direction of a Source of Sound.* Transactions of the Musical Association, 1876.

[2]The sound wave coming directly to both ears from a single source could show differences only in complexity, intensity, and phase. The first of these differences is ruled out of consideration by the use of the tuning-fork; the second, by his mathematical calculations.

whole sound mass seems to be transferred alternately from one side to the other. In order to interpret these results, he conducted a second series of experiments. The following results were obtained. (1) It was shown that the transference of the sound from one side to the other came directly after ("followed") the maxima and minima of sound as heard by a second observer, for whom the beats were allowed to occur. This established a correlation between the maximal changes in phase of the sound-waves received by the two ears and the phenomenon of transfer. (2) It was found, in addition, that when the wave of greater frequency was received by the right ear, for instance, the transfer to right occurred directly after agreement of phase, and the transfer to left came directly after the maximal opposition of phase. "The transitions between right and left effects correspond to agreement and opposition of phase, not usually recognized. When the vibration on the right is the quicker, the sensation of right follows agreement of phase, and (what is better observed) the sensation of left follows opposition of phase." The writer interprets this quotation to mean that the sound is heard on the right from agreement to opposition of phase, and on the left from opposition to agreement. Now a consideration of the phase relationships of two sound-waves differing in frequency shows that the wave of greater frequency leads in phase from agreement to opposition, and the wave of lesser frequency leads from opposition to agreement.[1]

[1]The writer can best show in the following manner what he conceives Rayleigh to mean by leading in phase. The vibrating particles forming each sound-wave execute simple harmonic motion. They may thus, in each case, be considered as moving on the circumference of a circle whose diameter is equal to the amplitude of vibration. For the sake of ready comparison, their amplitudes of vibration may be assumed as equal; and both may be considered as moving on the circumference of the same circle,

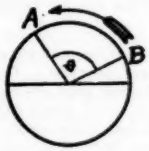

FIG. 1

FIG. 2

but at different rates of speed. Taking any two corresponding particles of the two waves, he considers that when the angle θ (the angle separating the radii at the outer termini of which the two moving particles are located, measured in the direction in which the particles are moving) is less than 180°, the particle moving at the greater rate of speed, considered with reference to the direction in which both are moving, will be ahead of the

Thus it can be inferred that the sound was referred throughout in these experiments to the side receiving the wave leading in phase. Rayleigh proposes to make of this a localizing clue, and applies it as a principle of explanation to the phenomenon of localization, as ordinarily observed, for all tones of low pitch. For example, when the source of sound is situated to the right or left of the median plane, the sensation is referred to the right or left, as the case may be, because at any given instant the wave acting upon the ear in question leads in phase the wave acting upon the other ear. And when the source is in the median plane, the sound is referred to that plane because the wave reaches the two ears in phase agreement. Continuing his experiments with forks of higher pitch, Rayleigh finds that the right and left effects occur without considerable diminution up to pitches of 320 vibrations per second. At this point the phenomenon begins to become indefinite and confused. After careful variations of his conditions, he concludes that 768 vibrations per second furnish the limit beyond which no trace of the effect is observed.

In a later report of work,[1] Rayleigh says: "When the sounds proceed from tuning-forks vibrating independently, the phase differences pass cyclically through all degrees, and if the beat be slow enough there is good opportunity for observation. But it is not possible to stop anywhere, or in some uses of the method to bring into juxtaposition phase relationships which differ finitely." He then describes a method of experimentation which allows any particular phase relation to be maintained at pleasure. Two telephone receivers were used as sources of sound. They were excited by a revolving magnet which acted indirectly upon two coils, one in each of the telephone circuits. The planes of the coils were vertical, their centres being at the same level as the magnet. One was fixed, and the other was so mounted that it could revolve about an axis coincident with that of the magnet. The angle between the

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slower particle; and, conversely, when the angle θ is greater than 180°, the faster particle will always be behind the slower particle. To illustrate (Fig. 1), let A represent the position of the faster particle on the circle of reference and B the position of the slower particle, both moving in a counter-clock-wise direction. When angle θ is less than 180°, A will be ahead of B; but when angle θ is greater than 180° (Fig. 2), B will be ahead of A. When angle θ is 180°, or 360°, neither will lead in phase.

The phase relations which any two particles vibrating at different rates will sustain at different times can be very prettily shown for class demonstration by two hands geared to move at the required speeds around a graduated dial.

[1]Lord Rayleigh: *Acoustical Notes, Sensations of Right and Left from a Revolving Magnet and Telephone*, Philos. Mag., Ser. 6, XIII, 1907, pp. 316-319.

planes of the coils represents the phase differences of the periodic electro-motive forces, subject it may be to an ambiguity of half a period, dependent upon the way the connections are made. If the circuits are similar, as is believed, the phase differences of the circuits and the electro-motive forces are the same. The circuit of one telephone included a commutator by means of which the current through the instrument could be reversed, corresponding to a phase change of 180°.

In conducting an observation, the sounds given by the two telephones are brought to equal intensities by a proper regulation of the distances between the magnets and the inductor coils. The telephones are thus brought into simultaneous action, and differences of phase are produced by rotating the movable coil; or if complete reversal is wanted, it may be got by means of the commutator. The results, he says, confirm those obtained with the tuning forks. A lead in phase was followed by the reference of the sound to the side receiving the wave which led in phase, and when the planes of the coils were parallel, *i. e.*, when the phases were in agreement or opposition, the sound was located in the median plane.

It may be of interest to note here the results of other observations made under conditions similar to those obtaining in Rayleigh's experiments. Thompson, working in the following way, reports beats, but makes no mention of right and left effects. (1) Tuning-forks, unresonated, were held one to each ear. (2) The sound of one fork was conducted to one ear through a rubber tube and the second fork was held to the other ear. (3) The forks were placed in different rooms and the sound was conducted separately through rubber tubes to the ears. The sounds had no opportunity of mingling externally or of acting jointly on any portion of the air columns along which the sound travelled. Speaking of this observation, he says (Philos. Mag., Ser. 5, III, 1877, p. 274): "The beats were most distinctly heard, and seemed to take place within the cerebellum." So W. H. Stone reports (*Ibid.*, p. 278) that he has been in the habit of using both ears, with a tuning-fork applied to each, in counting beats; and that he finds no difference between the results of this method and that of listening to both forks with one ear. Rayleigh (*On our Perception of Sound Direction*, Philos. Mag., Ser. 6, XIII, 1907, p. 220), speaking of Thompson's results, says: "In an observation of my own (Philos. Mag., Vol. II, 1901, p. 280; Scientific Papers, Vol. IV, p. 553), when tones supposed to be moderately pure were led to the ears by means of telephones, a nearly identical conclusion was reached. But although the cycle was recognized, in neither case, apparently, was there any suggestion of right and left effect. In repeating the experiments recently, I was desirous of avoiding the use of telephones or tubes in contact with the ears, under which artificial conditions an instinctive judgment would perhaps be disturbed. It seemed that it might suffice to lead the sounds through tubes whose open ends were merely in close proximity one to each ear, an arrangement which has the advantage of allowing the relative intensities to be controlled by a slight lateral displacement of the head toward one or the other source." This apparently was the only difference in the conditions between the experiments which gave beats and no right and left effects, and the experiments which gave right and left effects but not "plainly recognizable beats." Hermann (*Zur Theorie der Combinationstöne*, Pflüger's Archiv, XLIX,

1891, pp. 499-518) found that when the waves from two tuning forks were conducted one to each ear, he heard both beats and combination tones. In this case he supposed that the tones, through the mediation of the bones in the head, both acted together on each ear. No mention is made of right and left effects. Cross and Goodwin (Charles R. Cross and H. M. Goodwin: *Some Considerations Regarding Helmholtz's Theory of Consonance,* Proc. of the Am. Acad. of Arts and Sciences, XXVII, 1891, pp. 1-12) found beats and apparently the phenomenon of transfer from ear to ear. The meatus was closed with beeswax, leaving an air column between it and the tympanic membrane. The conduction under these conditions they think was directly to the tympanic membrane by means of the air column and not through the bones of the head to the middle ear or cochlea, because the sound of the fork, when the stem was touched to the wax, was heard long after it had ceased to be audible when the stem was touched to the pinna of the ear. It was also found that it could be heard longer when the stem was touched to the wax than when it was held against the teeth. When two small tonometer forks, tuned to four beats per second, were struck and their stems held against the teeth, "loud beats were heard in the ears. . . . . The forks were held in this position until the beats had entirely ceased to be audible, when they were removed and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats." The experiment was then varied slightly as follows. One ear only was closed with wax; the other was immersed in a large basin of water. "The experiment was then repeated as above, with the difference that one fork, instead of being touched to the ear, was touched to the marble basin, its vibrations being transmitted to the enclosed ear through the water. The same results were obtained as before." One of the conclusions drawn from these experiments, which is especially of interest relative to Wilson and Myers' experiments and their explanation of the localization of tones of low pitch (*Vide* this paper, pp .266-69), is that "aerial vibrations acting upon the ear are not transmitted through the skull, or bony parts of the head, from one ear to the other."

The phenomenon of beats has also been reported in this connection by the following experimenters, but none of them has mentioned a right-left transfer: Dove (Repertorium der Physik, Bd. III, 1839, S. 494; Pogg. Annal., CVII, 1859, S. 653); Seebeck (Pogg. Annal., LIX, 1841, S. 417; *ibid.*, LXVIII, 1846, S. 449; Akustik, Abschn. II, Gehler's Repertorium der Physik, 1849, S. 107); Mach (Wiener Sitzungsber, L, 1864, p. 356.); Stumpf (Tonpsychologie, Bd. II, S. 208, 458, 470); Bernstein (Pflüger's Archiv., LIX, S. 475); Ewald (Pflüger's Archiv, LVII, 1894, S. 80); Schaefer (Zeitschr. f. Psychol., u. Physiol., I, 1890, 81); and Melati (Philos. Studien, Bd. XVII, 1901, pp. 431-461). Sanford (Experimental Psychology, 1898, p. 82), however, working with forks beating once in two or three seconds notices, a shifting of the sound from ear to ear corresponding to the rate of beating.

It is with considerable reluctance that the writers present the preceding brief exposition of Rayleigh's theory and its experimental confirmation, because neither of these is worked out in the original article with sufficient detail to warrant the risk of a definite interpretation. In every case, therefore, where more than one interpretation has seemed possible, the one most favorable to the theory has been chosen. Until all the points involved both in the theory and in its confirmation have received more definite treatment by Rayleigh

the writers feel that positive criticism, either favorable or adverse, is out of the question. The following comments, however, may not be out of order. (1) The theory is purely physical. No attempt is made in any of the points to bridge over the gap between stimulus and sensation. In the treatment of lead and lag, for example, no consideration is given to what the ear, as a sense-organ, might be assumed to recognize as lead and lag. The mathematical propriety of Rayleigh's use of the terms is granted. And by mathematical definition, the faster wave will lead when angle θ is less than 180°. There will always be this characteristic phase relation between the waves coming to the two ears. But to grant that the ear can discriminate which is leading and which lagging, when no position or motion of any of the vibrating parts of the ear can, in the complete cycles of its changes, characteristically stand for lead or lag, and when no lead and lag aspect can be discovered in the sound sensation itself, seems to be ascribing to the auditory mechanism a logical or mathematical power which not even all educated beings possess as an item of culture. For example, when angle θ is say 160°, the faster wave may, at different times when this angle of separation occurs, be in every conceivable stage either of condensation or of rarefaction. During a part of this time, the slower wave will be at appropriate points in rarefaction when the faster is in condensation, and *vice versa;* and the rest of the time both waves will be either in rarefaction or condensation. Thus there is nothing in the position or motion of the vibrating structures of the ear that can be seized upon as characteristic of the lead or lag, as Rayleigh uses the terms, except a relation between direction of motion and angles of separation, and this is discovered only by a mathematical consideration of simple harmonic motion.[1] Just as Helmholtz's theory of vision has

[1]There is, for example, an alternative interpretation of lead and lag, which, it seems to the writers, the ear might more plausibly be assumed to recognize; namely, neither particle might be said to lead or lag unless both be moving either in condensation or rarefaction. Then the vibrating structures of the ear will be moving in the same direction, and at any given moment will be displaced in the same direction. Thus, as far as sensations of motion or position are concerned, if such sensations can be assumed for any of the vibrating structures of the ear, there would be a better chance for comparison than by the former interpretation. The writers, however, do not consider that this is the interpretation Rayleigh means to be taken for lead and lag; because (1) it is not the interpretation commonly given to the terms, and (2) in his experiments, it would leave the ear a part of the time in both halves of the cycle of changes without a localizing clue, for there will come times both from agreement to opposition and from opposition to agreement when one wave will be in condensation and the other in rarefaction, and conversely. Thus for a part of the time, in both cases of reference, right and left, there would be no localizing clue. This interpretation would

been called pre-psychological, so may this theory of localization be called pre-psychological.

(2) Speaking of the difference in phase inwhich the sound-wave from a source to the right on the aural axis would arrive at the orifices of the two ears, Rayleigh says:[1] "It is easy to see that the retardation of distance at the left ear is of the order of the semicircumference of the head, say one foot. At this rate, the retardation for middle C (C′=256) is nearly one quarter of a period; for C″ (512) nearly half a period; for C‴ (1024) nearly a whole period, and so on. Now it is certain that a phase retardation of half a period affords no material for a decision that the source is on the right rather than on the left, seeing that there is no difference between a retardation and an acceleration of half a period. It is even more evident that a retardation of a whole period or of any number of whole periods would be of no avail." In the preceding quotation just two stages of phase relationship are ruled out as furnishing no localizing clue; namely, a difference of a half period, and a difference of a whole period. A difference of a half period furnishes no clue, because the angular separation of corresponding particles of the two waves is 180°; hence it would be the same whether considered as acceleration or retardation. Similarly, a difference of a whole period furnishes no clue, because the angular separation of the corresponding particles of the two waves is 360°, hence would be the same considered either as acceleration or retardation. But in the scale of pitches, there are only certain members higher than 512 and 1,024 vibrations whose sound-waves coming from a given direction would always arrive at the two ears with a difference of 180° or 360°. Phase difference, so far as can be readily seen, should furnish a clue for the localization of the higher just as well as of the lower pitches. There seems to be no good reason, then, for making the direct recognition of phase difference a localizing clue for the lower pitches only, and for giving over to difference of intensity the exclusive rôle for the higher pitches. And again, if a direct recognition of phase difference be a localizing clue, it is not easy to see why the right and left effects in Rayleigh's experiments should suffer considerable diminution when the tones were as high as 320, and should cease entirely near 768 or above. It is just as clear that the faster wave will lead from agreement to opposition and the slower from opposition to agreement in this case as in the case of lower tones. And if the tones

work even worse in localization as ordinarily observed, for in every direction there would be certain distances from the ear for which there would be no localizing clue.

[1] *Op. cit.* p. 218.

were near together in pitch, the transitions from right to left effects should have come just as slowly and should therefore have been just as easily observed. That is, if 768 and 769 forks were used, for example, the change from agreement to opposition and from opposition to agreement, and the corresponding right-left, left-right transfers, should have occurred only once per second, just as would have occurred with forks of 128 and 129 vibrations per second. (3) Phase difference must make itself felt in consciousness either by means of some change in the sound sensation itself, or by setting up some new sensation alongside the sound sensation; for example, a sensation of position or movement of some of the vibrating structures of the ear. Wilson and Myers[1] conclude that phase difference makes itself felt as a difference in the intensity of the sound heard by the two ears. They explain the localizations in Rayleigh's experiments in terms of this difference in intensity produced by cyclic changes of interference between the sound-waves coming directly to the two ears and those transmitted from one ear to the other through the bones of the head. Rayleigh, however, obviously considers that the effect of phase difference is extra to any differences that may occur in the intensity aspect of the sensations given to the two ears. The question arises as to whether either explanation can be applied further than to the special phenomenon created by the experimental conditions under which they worked. This point will be taken up in a later section of this paper. (4) Until more sensitive tests than those conducted by Rayleigh are made to find out the relative sensitivity of direction-discrimination for low and high tones, it is not demonstrated that there is any need for a supplement to the intensity theory, to account for the localization of low tones.

Later, in 1907, More and Fry[2] also attempted to show that phase difference serves as a clue for the localization of sound. They worked with tuning-forks of 320 and 512 vibrations per second. The observer was seated at the centre of a large circle marked on the floor of a room. The zero point in the circle was taken directly behind the observer, the 180° point in front, and the 90° points at the sides. A glass funnel 13.5 cm. in diameter was mounted horizontally on a table at the zero point, about 7 ft. behind the observer. Heavy rubber tubing with an inner diameter of about 1.2 cm. connected the funnel with the stem of a glass Y-tube, on the two branches of which rubber tubing of the same size was fitted. These branch tubes ended in glass tubes bent so as to fit into the ears of the ob-

[1] *Vide* pp. 267-68, this paper.
[2] *Op. cit.*

server. Each of the branch tubes was cut in two at the middle; and by inserting pieces of glass tubing, the experimenter readily altered their lengths without the listener's being aware of the change. Fourteen observers were used. The sound was given at the mouth of the funnel, and the observer was asked to indicate the direction from which it came. This direction was estimated by means of the graduated circle at the centre of which the observer sat. Before each observation, the length of one of the tubes was changed ⅛, ¼, ⅜, ½, ⅝, ¾, or ⅞ of a wave-length of the sound used as stimulus. The results, expressed in general terms, showed that when the tubes were exactly equal in length, the sound seemed to come directly from behind; but if one tube was made shorter than the other, by as much as 2 cm., the sound was referred to the side having the shorter tube. These results were taken to indicate that sound is localized by a direct appreciation of the difference of phase of the waves coming to the two ears.

With regard to this work, the writer would point out the following facts. (1) The forks employed were both above the pitch limit at which Rayleigh claims the difference in intensity becomes too small to serve as a localizing clue. Hence these writers, in their support of the phase difference theory, work with tones for which Rayleigh claims the localizing clue is difference of intensity and not of phase. (2) Their results, stated in general terms, do not seem to have any differential value. When one tube is made shorter than the other, the stimulus received by the ear on that side is more intensive than that received by the other ear; hence, by the intensity theory, as well as by the phase difference theory, the sound should be referred to that side. The sounds worked with had a wave-length of 64 (512 fork) and 104 (320 fork) cm. per sec. A change in the length of one of these comparatively short tubes by from ⅛ - ⅞ of a wave-length of these sounds would produce a considerable change in the ratio of the distances the sound had to travel to reach the two ears; hence it would produce a considerable difference in the energy of the stimulation given. As nearly as the writer can determine, by comparing the measurements and localizations given by More and Fry with the measurements of the direct paths of transmission of the sound to the two ears from these locations in his own sound-cage, the change of ratio is quite of the same order in the two cases.

(3) Although it may be considered that the results in general are not differential as between the two theories, More and Fry find a crucial argument in the fact that as one tube was progressively made longer than the other, there was not a constant increase in the displacement of the sound toward the ear sup-

plied with the shorter tube. In a few cases, for example, the 90° displacement was made when the change in the length of the tube was only ⅝-¾ of the wave-length; and from that point on to a change of a whole wave-length, there was even, in certain cases, a decrease in the angle of displacement. This, they say, on the basis of the intensity theory, ought not to be. There should be, following a change in the relative lengths of the tubes, a regular increase of displacement. To quote: "If it were a question of change of intensity, the change in direction would increase continually and not reach an angle where further increase in the length of the tube produces either a doubtful increase in angle or even at times a decrease." In reply to this, it may be pointed out (*a*) that a sound cannot be displaced more than 90° to either side, and the ratio of the length of the tubes which gave their observers a displacement of the sound 90° to the right was of the same order as the ratio of the lengths of the direct paths of transmission to the two ears when the sound is actually given 90° to the right in the sound-cage used in our own experiments. Moreover, as localizations ordinarily occur in everyday life, the ratio of the distances the sound travels in order to reach the two ears is, in most cases, not even so great as it is with the Titchener sound-cage. The above result, then, is all that could be expected by the intensity theory. (*b*) To account for the decrease in the angle of displacement in a few cases mentioned, it may be said that when the ratio of intensity, by the conditions of the experiment, is made to exceed any ratio that could occur in our daily life, there is no basis for the association of any given direction with that ratio; hence, once this limit is exceeded, regularity of results should not be expected. Under such conditions, one might expect almost any irregularity, but certainly not regularity.

(4) The discrepancy between More and Fry's and Wilson and Myers's[1] results should not be ignored. In both cases apparently the same method of working was used, yet very different results were obtained. For More and Fry's observers a change in the relative lengths of the tubes was followed uniformly by a displacement of the sound toward the side of the shorter tube; while for Wilson and Myers's observers, changes in the length of the tubes were followed by a cycle of changes of localization,—first to the side of the shorter tube, then to the median plane, to the side of the longer tube, back to the median plane, and so on. So great a discrepancy cannot but throw both sets of results open to question, until more work is done under similar conditions.

[1] *Vide* this paper, pp. 266-67.

In October, 1908, Wilson and Myers reported a series of experiments suggested by the work of Rayleigh. The general plan of experimentation was similar to that used by More and Fry. The apparatus,[1] however, was more carefully designed, and the effect of changes in the ratio of the length of conducting paths was possibly more minutely tested out.[2] The sound was led to the ears separately by the two arms of a rectangle made of tubes of glass and brass joined at the corners by India rubber tubing. The observer's head, in position, occupied the mid-point in the back of this rectangle. Opposite his head, in the mid region of the front of the rectangle, a section 120 cm. long was removed and in its place a section of T-tubing was inserted, the horizontal arm of which was small enough in diameter to slide freely within the main tube, and long enough to permit the perpendicular arm to be moved 60 cm. on each side of the median plane. The perpendicular arm ended in a funnel-shaped collector, in front of which the tuning-fork was sounded. A centimeter scale was mounted behind the T-tube, upon which could be read the displacements of the perpendicular arm to the right or left of the median plane. The tubes leading to the ears ended in wooden receivers or ear-caps, which were pressed against the observer's head, and held in place by retort stands fastened to the table in front of the observer. The movements of the experimenter were shut off from the observer's view by a large screen across the median plane. When the funnel receiving the sound was placed in the median plane, the arms leading to the ears were of equal length, in most cases 317 cm. each. In this position it was found that the sound was located in the median plane. When the funnel was displaced by amounts varying from $0—\lambda/4$ ($\lambda =$ wave length) or $\lambda/2—3\lambda/4$, the sound was referred to the side toward which the displacement had been made. When it

[1]Apparatus somewhat similar to that used by Wilson and Myers is described by Urbantschitsch (Arch. f. d. ges. Physiol., 1881, XXIV, 579-585).

[2]Sylvanus Thompson (Philos. Mag., Ser. V, Vol. VI, 1878, pp. 386–387) worked in much the same fashion as Wilson and Myers, but with less minute measurement. The ends of a curved copper wire 3 ft. long, bent into two rings, were inserted one into each of the observer's ears. It was found that when the stem of a vibrating fork was set on this wire at the mid point, the sound seemed to come from the ends of the wire in each ear. A change of an inch and a half from this position produced a sufficient difference in the length of the path travelled by the sound to cause it to reach the two ears in complete difference of phase. Given in this position, the sound seemed to come from the back of the head. When the sound was given in intermediate positions, the effect was of a mixed character; part of the sound seemed as if located in the ears themselves, and part of it seemed to come from the back of the head. No change in this result was observed with forks of different pitches, providing that the proper differences in length of path were chosen.

was displaced by amounts varying from $\lambda/4$—$\lambda/2$ or $3\lambda/4$—λ, the sound was referred to the side opposite to that toward which the displacement had been made. The same relations between the reference of the sound and the displacements of the stimulus were observed to hold for simple multiples of λ. Thus it was found that when the stimulus is displaced either to the right or left of the median plane, the sound is successively referred, as the displacement is increased, first to the side toward which the displacement is made, back to the median plane, to the opposite side, and back to the median plane, repeating the cycle when the displacement reaches an amount exceeding one wave length.

In explaining these results, Wilson and Myers agree with Rayleigh that the localization of tones of low pitch is dependent upon the difference in the phase of vibration in which the sound waves reach the two ears. They do not, however, believe it is necessary to assume that the localizing clue is a direct recognition of phase difference by the two ears. They contend that "while binaural differences in phase are a primary cause of the observed lateral effects, these effects are ultimately referable to binaural differences in intensity." The stimulus in either ear is a resultant of two vibrations, one communicated directly to the ear, the other indirectly, through bone conduction from the opposite ear. The resultant, now stronger in one ear, now in the other, now equal in both ears, because of progressively changing phase differences between the direct and transmitted waves, determines the direction-reference. Suppose, as in the case of the Rayleigh experiments, two sound-waves of equal amplitude but of unequal frequency enter the two ears. Then from agreement to opposition the faster wave will lead the slower in phase, and from opposition to agreement the converse will be true. When the faster is leading, the resultant of the direct and transmitted waves in the ear receiving the faster wave will be of greater intensity than in the ear receiving the slower wave, and the total sound mass will be referred to that side. Similarly, when the slower leads in phase, the resultant will be stronger in the ear receiving the slower wave, and the localization will occur on that side. When, however, the two waves are in opposition or agreement, the resultants in both ears will be equal, and the localization will be in the median plane, as the intensity theory requires. Since, as they believe, this hypothesis satisfactorily explains the results of Rayleigh's experiments, they do not think that these experiments should be considered as affording differential evidence for the phase-difference theory. Nor do they claim differential value for their own experiments. With reference to physical features alone, both hypotheses are

capable of explaining both sets of results. Wilson and Myers, however, claim an advantage for their hypothesis on the ground of its greater plausibility and of the auxiliary facts that can be cited in its support. On the ground of plausibility, they maintain that their hypothesis is in better accord with the prevailing conception of the origin and nature of nervous impulses. For example, Rayleigh states:[1] "It seems no longer possible that the vibratory character of sound terminates at the outer ends of the nerves along which the communication with the brain is established. On the contrary, the processes in the nerve must in themselves be vibratory, not in the gross mechanical sense, but with the preservation of the period and retaining the characteristic of phase—a view advocated by Rutherford as long ago as 1886." Wilson and Myers believe that there is too much evidence of the specific functioning of end-organs to be overweighed by the results of Rayleigh's experiments. A special sense-organ may be excited not only by the stimuli to which it is especially adapted to respond ("adequate" stimuli), but also by "inadequate" stimuli; for example, electrical, chemical, and mechanical. "Inasmuch as the sensations are similar despite the diverse character of the stimuli, we have hitherto believed that the impulses ascending a sensory nerve depend on the mode of response of the end organ and not directly on the character of the stimulus." By way of auxiliary facts, they cite the results of Mader,[2] the tuning-fork experiment of Weber, and its modification suggested by Schaefer,[3]

[1] Philos. Mag., Ser. 6, Vol. XIII, 1907, pp. 224-225.

[2] In Mader's experiments (Sitzungsber. d. kais. Akad. d. Wissens., Wien, 1900, Bd. CIX, Abth. 3, S. 37-75) two tones of nearly identical pitch were separately led one to each ear hole of a skull. A microphone applied to the roof of the skull gave evidence of beats. This is cited as presumptive evidence that the tones were actually passing across the roof of the skull from one ear to the other. (Both tones, however, were generated in the same room, hence there is no guarantee that the microphone was not acted upon by beats in the air wave.)

[3] Schaefer, K. L.: Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg. 1891, Bd. II, S. 111-114. Wilson and Myers (*Op. cit.*, p. 318) describe this experiment as follows: "A fork, fixed at some distance from one side of the observer, is very gently struck. The observer listens, and notes when the dying tone has become quite inaudible. He then inserts an appropriately attuned resonator into the ear which is nearest the fork; whereupon, the tone is at once softly heard again on that side, as if it came from the resonator. If the meatus of the more distant ear be now closed, the tone becomes at once stronger, and its localization approaches the median plane. If the meatus be then re-opened, the tone immediately leaps back again to the ear in which the resonator is inserted."

Mach has suggested that when a tuning-fork is placed on the vertex, and the meatus of one ear is closed, the tone is localized to that side, because the sound travelling by bone conduction to that ear not only stimulates the cochlea, but sets up in the meatus vibrations which are reflected back and intensify the sound in that ear.

as evidence of bone conduction[1] under conditions similar to those in their own and in Rayleigh's experiments.[2]

As was stated earlier in the paper, this review will be concluded by a brief résumé of the arguments that have been advanced, up to the present time, for the binaural ratio as a factor in auditory localization. They are as follows. (1) Confusion points are found in the median plane and in the planes parallel to it on either side (Rayleigh,[3] von Kries and Auerbach,[4] Pierce,[5] and Starch[6]). (2) Monaural localization is inferior to binaural localization. (Politzer,[7] Arnheim,[8] Preyer,[9] Bloch,[10] von Bezold,[11] Smith,[12] Angell and Fite,[13] and Starch.[14]) Politzer, Arnheim, Preyer, and Bloch worked with cases of monaural hearing artificially produced; von Bezold, Angell and Fite, and Smith worked with pathological cases. Starch worked with two observers in which the defect was artificially produced, and two in which it was pathological. For Politzer, Arnheim, Preyer, Smith, and Angell and Fite, the test used was accuracy of localization; for Bloch it was the size of the j. n. d.; and for Starch it was both accuracy of localization and the size of the j. n. d.

(3) The greatest accuracy of localization occurs at points where a change of direction produces the greatest change in

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[1]Against bone conduction *vide* the experiments and conclusions of Cross and Goodwin (this paper p. 260.)

[2]There may be cited additional casual advantages, so obvious, however, as to be scarcely worthy of mention. (1) Wilson and Myers's explanation does not involve the assumption of any new power on the part of the ear, hence it has the advantage of systematic simplicity. (2) Introspective analysis does not show any aspect of the sound sensation, or any new sensation simultaneous with the sound sensation, corresponding to difference of phase.

[3]Rayleigh, Lord: *Acoustical Observations, Perception of the Direction of a Source of Sound*, Philos. Mag., Ser. 5, Vol. III, 1877, pp. 456-458.

[4]*Op. cit.*, p. 330, 336.

[5]*Op. cit.*, pp. 56-78.

[6]Psychol. Rev., Monog. Suppl., Vol. IX, 1908, p. 53.

[7]*Op. cit.*, p. 231-236.

[8]Arnheim: Beiträge zur Theorie der Lokal. von Schallemp. mittl. der Bogengänge. Diss, Jena, 1887.

[9]Preyer: *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Pflüger's Archiv, XL, 1887, pp. 618-619. It will be remembered, however, that Preyer and Arnheim believed that the localization is in terms of space feelings aroused directly by the action of the sound wave upon the semicircular canals.

[10]*Op. cit.*, p. 59-73.

[11]*Op. cit.*, p. 486-487.

[12]*Op. cit.*, p. 542.

[13]*Op. cit.*, pp. 225-246 and 449-458. Angell and Fite claim that a considerable degree of inferiority of monaural hearing exists only in the case of unpracticed monaural subjects. Monaural subjects can be practiced up to the point of localizing almost as well as the unpracticed normal subject.

[14]Psychol. Rev., Monog. Suppl., IX, 1908, pp. 40-48.

the binaural ratio, *i. e.*, in front and back near the median plane; and the poorest localization occurs where a change of direction produces the least change in the binaural ratio, *i. e.*, at the sides, near the aural axis[1] (Bloch,[2] and Starch[3]).

(4) A difference in the amount of collection of the sound-wave at the orifices of the ears determines the localization to the side receiving the greater energy of the wave (Thompson,[4] and Kessel[5]).

The total sound-mass is referred to the side of the stronger stimulus when two sounds, one stronger than the other, are given to the two ears. (Steinhauser,[6] Tarchanoff,[7] and Matsumoto[8]).

When two tuning-forks sounding with equal intensity are placed one on each side of the head, but one nearer to the ear than the other, the total sound-mass is referred to the side on which the nearer fork is located (Stenger[9]).

When two tuning-forks, sounding with equal intensity and located in the aural axis on either side at the same distance from the ears, are swung in unison from left to right and right to left, a transfer in the localization of the total sound mass takes place following the rhythm of the swing. When both are swinging to the left, the sound is referred to the right, and, conversely, when both are swinging to the right, the localization is on the left. When both are swinging with equal speed in opposite directions, the localization is in the median plane (Fechner[10]).

When two vibrating bodies are in contact with the head or very near to it, and the energy of vibration is unequal, the

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[1]Starch (*Op. cit.*, p. 52.) phrases this as follows. "The accuracy of localization is greatest where slight changes in the ratio are most readily perceived, *i. e.*, in front and back. Localization is poorest where changes in the ratio are not so easily perceived, *i. e.*, on the sides, in the region of the aural axis."

[2]*Op cit.*, pp. 31, 35.

[3]Psychol. Rev., Monog. Suppl., Vol. VI, No. 4, 1904-05, pp. 11-12 and 44; *ibid.*, Vol. IX, No. 2, 1908, pp. 52-53.

[4]Thompson: Philos. Mag., Ser. 5, Vol. VIII, 1879, p. 386; *ibid.*, Vol. XIII, 1882, p. 412.

[5]*Op. cit.*, p. 120.

[6]*Op. cit.*, pp. 188-189. Steinhauser used as the source of sound an instrument called by him the homophone. This instrument consisted of two organ pipes of the same pitch, one of which was supported near to each ear on the level with it. The intensity of the sound was regulated by means of valves controlling the pressure of the air blast used to excite it.

[7]*Op. cit.*, p. 354.

[8]*Op. cit.*, p. 18. Matsumoto used two telephone receivers placed opposite the two ears, one on each side. The intensity of the sound in each ear was controlled by a sliding inductorium.

[9]Stenger: *Op. cit.*, p. 223.

[10]Fechner, G. T.: *Op. cit.*, p. 543.

sound is localized within the head but is referred to the side receiving the greater energy of vibration (Urbantschitsch,[1] and Thompson[2]).

When the stem of a vibrating tuning fork is placed on the vertex of the skull, the tone is localized somewhere midway between the two ears; but if the meatus of one ear is stopped and the wave is reflected back toward the internal ear, the sound is transferred immediately to that side (Weber[3]).

When a tuning-fork is faintly sounded on one side and heard by the ear on that side by means of a resonator, the sound is referred to that side; but when the meatus of the opposite ear is stopped, the sound approaches the median plane (Schaefer[4]).

One of two fusing sounds may be placed in either of the lateral quadrants without altering the localization of the fusion (Pierce[5]).

II. Experimental

A. The demonstration of the binaural ratio as a factor

(a) *Lines of Argument.* The object of this paper is to add three lines of argument to those mentioned above. (1) Observers having a natural difference in sensitivity of the two ears show a constant tendency to displace the source of sound toward the axis on the side of the stronger ear; and, conversely, observers without this difference in sensitivity show no consistent tendency toward right or left displacement. (2) Changes in the ratio of sensitivity of the two ears, produced by plugging either ear, are followed by corresponding displacements of the sound toward the more sensitive ear. (3) A natural tendency toward right or left displacement can be corrected by making the proper change in the ratio of sensitivity of the two ears.

The principle involved in the second argument is not entirely new.[6] It aims at a direct and systematic correlation

[1]Urbantschitsch: *Lehre von der Schallempfindung*, Pflüger's Archiv, XXIV, 1881, 579.

[2]Thompson, *On Binaural Audition*, Philos. Mag., Ser. 5, Vol .IV, 1877, pp. 274-276; *Phenomena of Binaural Audition*, *ibid.*, Ser. 5, Vol. VI, 1878, pp. 383-391.

[3]Weber: *Op. cit.*, p. 42.

[4]Schaefer: *Op. cit.*, pp. 111-114.

[5]Pierce: *Op. cit.*, pp. 63 and 147.

[6]It might probably be said that the principle involved in the first line of argument is also not entirely new. Results of monaural localization have been reported by numerous investigators; and occasional mention has been made of a suspected influence of difference in sensitivity of the two ears upon the results obtained in cases of binaural localization. (For the best example of this, *vide* Seashore: *Localization of Sound in the Median Plane*, Univ. of

between the intensity of the sound as heard by the two ears, and the direction in which it is referred. This has not been attempted before, although it has been shown more or less definitely that a difference in the energy of the sound-wave delivered to the two ears affects localization. Thompson,[1] for example, with his pseudophone, tried to produce a difference in the energy of the stimuli given to the two ears by means of the way in which the shell-shaped collectors were turned with reference to the direction of the stimulus, and to show thereby that the localization was determined toward the side receiving the stronger stimulation. Though the underlying principle of this general line of argument is not new, the writers have followed it up for the following reasons. (1) Its possibilities for demonstrating the binaural ratio as a factor in localization have not been fully utilized. (2) In order to confirm the intensity theory, it is necessary to show a definite correlation between the ratio of intensity of sensation, and the direction in which the sound is referred. The method of varying the sensitivity of the two ears gives a much safer and more direct means of establishing this correlation than is given by varying the intensity of the stimuli. The writers find, for example, that the ears of many people vary greatly in sensitivity. In fact, so far as his experience goes, it is more common to find a difference than to find the ears of

Iowa Studies in Psychol., 1899, II, p. 49). But for theory, monaural hearing presents a very different case from difference in sensitivity in binaural hearing (when working with monaural hearing the binaural factors drop out entirely); and, furthermore, no systematic attempt has ever been made to utilize differences in sensitivity as a means of demonstrating the influence of the binaural ratio.

For reports on monaural localization, see Politzer (*Loc. cit.*); Preyer (*Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Pflüger's Archiv, XL, 1887, S. 586); Arnheim (*Beiträge zur Theorie der Lokal. von Schallempf. mittelst der Bogengänge*, Diss. 1887.); Münsterberg (*Raumsinn des Ohres*, Beiträge zur Exp. Psy., Bd. II, 1889, S. 182), von Bezold (*Urteilstäuschungen nach Beseitigung einseitiger Harthörigkeit*, Zeit. f. Psy. u. Physiol. II, 1890, S. 486); Schäfer (*Ein Versuch über die interkranielle Leitung leisester Töne von Ohr zu Ohr*, Zeit. f. Psy. u. Physiol. II, 1891, S. 111); Smith (*How do we Detect the Direction from which Sound Comes?* Cincin. Lancet-Clinic, N. S. XXVIII, 1892, 542); Angell and Fite (*The Monaural Localization of Sound*, Psy. Rev. VIII, 1910, 225-266, and *Further Observations on the Monaural Localization of Sound, Ibid.*, 449-458); and Starch (*Perimetry of the Localization of Sound*, Psy. Rev. Mon. Supp. IX, 2, 1908, 1-55). For mention of a suspected influence of difference in the sensitivity of the two ears upon localization, see Pierce (*Op. cit., p.* 106), and Starch (*Op. cit.*, pp. 43-44). An influence of difference of sensitivity of the two ears is suggested by Arnheim (*Op. cit.*, p. 10, note) to explain his results when working with monaural hearing artificially produced. The left ear was found to have the superior power to localize correctly. He thought this might be due to its better blood supply. Pierce speaks against this part of Arnheim's work (*Op. cit.*, p. 107).

[1] *Op. cit.*, pp. 385-390.

approximately equal sensitivity.[1] A method, then, which seeks to vary the intensity of the sound as heard by the two ears, and does not take into account their probable difference in sensitivity, is obviously at fault; for there is no guarantee that a slight difference in the energy of the sound-waves delivered to the two ears, such as was produced by a different setting of the small shell-shaped collectors of Thompson's pseudophone, will be sensed as the relative intensities of the stimuli would indicate. If one ear should be more sensitive than the other, the two sound-waves, although one is stronger than the other, may be sensed as equal in intensity; or the ratio indicated by the stimuli may be reversed. Because, then, of the common occurrence of a natural difference in sensitivity between the ears, a method that attempts to measure the ratio of intensity of the sensations experienced by the ratio of intensity of the stimuli given, does not afford a safe basis for a correlation between the intensity of the sound as heard by the two ears and its localization. (3) Apart from the propriety of method, a third reason for continuing this line of attack is that the results reported from it have been too vague and uncertain to give much support to the intensity theory. For example, (a) the shell-shaped collectors in Thompson's pseudophone were assumed to collect more sound when given one direction than when given another; but there was no objective determination of how much they varied the intensity of the wave impinging upon the tympanum, or whether they varied it at all. No proper basis was laid even for a correlation of ratio of intensity of the two stimuli with the direction in which the sound was referred. (b) The method used for recording Thompson's localizations was indefinite, and his report of results is vague and uncertain. In short, a characteristic displacement of the sound toward the side receiving the more intensive stimulation is expressed (in the paper of 1879) as a matter of belief rather than as an established fact.[2]

(b) *Description of Method and Apparatus.* The writers were led to make this study by the results of tests they had been conducting on the relative sensitivity of the two ears in different people. The large number of subjects who were found to have a marked difference in sensitivity seemed to make possible a determination of whether or not the hearing of a

[1] Fechner, investigating the relative sensitivity of the two ears (*Ueber die ungleiche Deutlichkeit des Gehörs auf linkem und rechtem Ohre*, Berichte der kgl. Sächs. Ges. der Wiss. Math.-phys. Classe, XII, 1860, 166-174), found that out of 215 persons examined only 51 had ears of approximately equal sensitivity.

[2] *Op. cit.*, pp. 388-390.

sound more strongly by one ear than the other leads to constant errors in localization. Assuming that the binaural ratio is a factor in localizing, there seem *a priori* to be two possibilities relative to this question. (a) The subject so affected may, in proportion to his defect, show a constant tendency to displace the sound towards the aural axis on the side of the more sensitive ear, or (b) this tendency may have been wholly or in part corrected in the subject's past experience, through the influence of the space reference of other sense organs, in such a fashion that the false ratio has shown a tendency to become associated with the true reference. If so, the amount of the constant error probably should sustain some ratio to the length of time the defect had existed. If, for example, it were congenital or contracted very early in life, one might expect less error in localization than if it were of recent occurrence. Unfortunately the subjects, up to the time of these tests, were not aware of their defect, consequently no data of the sort were available. The effect of recency of defect, however, came out strongly in the experiments in which the ratio was varied by artificial means. Differences in sensitivity, artificially produced, exerted a much more marked influence upon localization than did approximately equal differences due to natural defect.

Artificial variations were produced both upon defective subjects and upon subjects in whom the sensitivity of the two ears was approximately equal. In both cases the effect was marked and consistent. In case of the normal subjects, in turn first one ear was made more sensitive, then the other. In case of the defective subjects three variations were introduced. (a) The defect was exaggerated, *i. e.*, the difference in sensitivity was rendered greater by plugging the less sensitive ear.[1] (b) An effort was made to correct the defect by decreasing the sensitivity of the stronger ear. Our object here was to establish a ratio of sensitivity that should eliminate any approach to a constant tendency to displace the sound in either direction. This was a procedure involving many trials and much patience. Our first idea was that this result should be attained by equating the sensitivity of the two ears. This device, however, in case of the subjects

[1]In all cases of plugging one ear, care was taken that monaural hearing was not produced. Before the observation began, both ears were firmly closed by the hands or some other effective means, until the stimulus used in the localizing experiments could no longer be heard, whatever position it might be given in the system used. The plugged ear was then uncovered, and the stimulus given at the most remote positions to be used in the experiment which was to follow. In no case was the observer unable to hear the sound.

used, overshot the mark. When the sensitivity of the stronger ear was decreased until it approximately equalled that of the weaker ear, a constant tendency to displace towards the normally weaker ear resulted. A compromise position then had to be sought. We finally succeeded in getting, with each subject, a ratio such that the error, roughly speaking, was apparently about as much and as frequently to one side as to the other of the true location. (c) A third variation was to plug the stronger ear until it became less sensitive than the weaker ear.

In all of our experiments, in order to guard against a wrong correlation of ratio with localization error, due to possible variations in sensitivity from day to day, or even from the beginning to the end of the experiment, sensitivity determinations were made at each sitting both immediately before and immediately after the localizing tests were made.

The ratio of sensitivity was obtained by comparing the limen of sound for the two ears. The observer was blindfolded and required to bite an impression previously made in a wax mouthboard. A wooden bar was supported in the line of the axis of the two ears, one end reaching as near as possible to the ear that was being tested. The other ear was carefully plugged. A watch was carried out along the bar until the limen was reached. An average of the results obtained by the method of approach and recession was taken as the final liminal distance, and the ratio of these distances was taken to represent the ratio of sensitivity of the two ears. To make sure that the plugged ear was not functioning in these tests, the watch was held as closely as possible to it without touching the lobe and the observer required to tell whether it could be heard or not.

The localizing experiments were carried on by means of the Titchener sound-cage. A Galton whistle set at 20,000 vibrations per second was used for the stimulus. As to devices for indicating the location of the sound, the pointing method, the chart method, and a combination of the two were used at different times. The authors have not made an exhaustive study of the relative merits of these methods but is inclined to prefer, on the basis of what they have done, a careful use of the pointing method alone. Any method involving the use of the chart has, in his experience, fostered a tendency on the part of the observer to delay the reference, to become uncertain and hesitating, to reason and debate with himself rather than to let whatever sensory mechanism for localizing with which nature may have provided him work itself out automatically. The errors arising from this tendency are, in our opinion, greater and more capricious than those from

wrong pointing, if proper care is exercised to make sure that the observer is pointing as he intends to point. The question of methods of recording, however, makes little difference for or against the validity of our demonstration; for (a) the constant displacement tendency appears whatever the method used, and (b) no method, however comprehensive its faults, could account for the consistent throw in opposite directions in case of different observers, or in the case of the same observer, when first one ear then the other is made more sensitive.

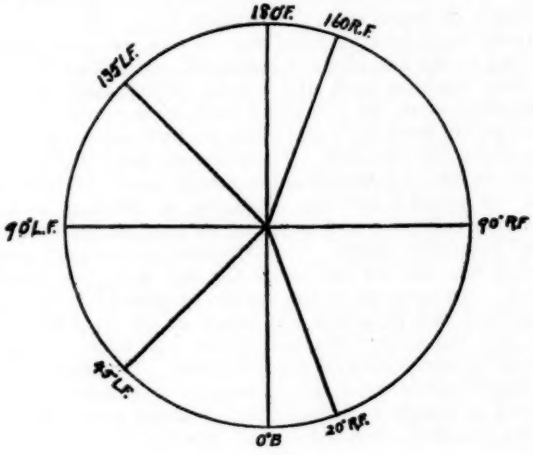

FIG. III

0° B.	180° F.
45° R. F. or L. F.	135° R. F. or L. F.
50° R. F. or L. F.	130° R. F. or L. F.
60° R. F. or L. F.	120° R. F. or L. F.
70° R. F. or L. F.	110° R. F. or L. F.
150° R. F. or L. F.	30° R. F. or L. F.
160° R. F. or L. F.	20° R. F. or L. F.

Thus far results have been obtained from ten observers in the investigation proper. In addition, the writers have roughly used, at one time or another for two years, all of the variations as a part of the drill course in his undergraduate laboratory.

(c) *Results.* The following tables have been compiled from the results of three of these observers, who were selected as representative: the Misses Friend (*F.*), Root (*R.*), and

Sharp (*S.*). Tables I-XII, inclusive, show the results of the experiments devised to demonstrate the importance of the binaural ratio as a factor in auditory localization. In all the tables given throughout the paper, locations in the horizontal plane are expressed in terms of the readings of the Titchener sound-cage. In this system of reference, the zero point is placed in the median plane, behind; the 90° points in the aural axis, right and left; and the 180° point in the median plane in front. It was found more convenient, however, in the vertical planes, to deviate from the scale of the sound-cage; the zero point was taken in the plane of the aural axis, and directions were read 90° up and down. Displacements to right or left were estimated from the actual position of the stimulus, or from its corresponding point front or back, as the case happened to be. For example, a stimulus given at 160° right front (R. F.) might be referred by the observer either to 160° R. F. or to its corresponding point, 20° R. F., without the reference being considered a displacement toward either ear. But if a stimulus were given at 160° R. F. and

TABLE I

Observer *F.* Showing the influence of the binaural ratio upon the localization of clangs. Natural sensitivity series. Liminal distance for right ear, 40 cm.; for left ear, 40 cm. Ratio, Left: Right = 1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	50° R F	0° d	5°	
135° L F	0°	50° L F	8° d		5°
0° B	0°	0° B	5° d	0°	0°
135° R F	0°	125° R F	5° u	10°	
45° L F	45° u	50° L F	5° u		5°
180° F	0°	20° L F	0°		20°
135° L F	45° u	40° L F	20° d	5°	
45° L F	45° d	50° L F	40° d		5°
150° R F	0°	145° R F	0°	5°	
0° B	45° u	10° R F	0°	10°	
45° R F	45° u	50° R F	0°	5°	
180° F	45° u	0° B	40° u	0°	0°
135° R F	45° u	135° R F	30° u	0°	0°
150° L F	45° u	35° L F	25° u		5°
0° B	90° u	20° R F	20° u	20°	
70° L F	0°	70° L F	0°	0°	0°
50° R F	45° d	120° R F	5° d	10°	
70° R F	0°	80° R F	0°	10°	
0° B	0°	0° B	0°	0°	0°

Average displacement from median plane 2.1° (right).

were referred either to 140° R. F. or to 40° R. F., the reference would be considered a displacement toward the axis of the right ear. Fig. 3 shows a diagram of the horizontal plane of reference, with two pairs of corresponding points represented. Below it are given readings in degrees for the corresponding points used in the experiments.

Tables I, II, and III show the results for the natural difference in sensitivity of the two ears for Observers *F.*, *R.*, and *S.*, respectively. In each case, the liminal distance for the

TABLE II

Observer *R.* Showing the influence of the binaural ratio on the localization of clangs. Natural sensitivity series. Liminal distance: right ear, 18.5 cm.; left ear, 72 cm. Ratio, Left : Right = 4. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	25° R F	30° d	20°	
135° L F	0°	50° L F	30° d	5°	
0° B	0°	15° L F	50° d	15°	
135° R F	0°	40° R F	38° d	5°	
45° L F	45° u	60° L F	35° d	15°	
180° F	0°	35° L F	20° d	35°	
135° L F	45° u	70° L F	38° d	25°	
45° L F	45° d	55° L F	33° d	10°	
150° R F	0°	40° R F	42° d		10°
0° B	45° u	35° L F	35° d	35°	
45° R F	45° u	10° R F	45° d	35°	
180° F	45° u	30° L F	42° d	30°	
135° R F	45° u	15° R F	45° d	30°	
150° L F	45° u	40° L F	35° d	10°	
0° B	90° u	50° L F	10° d	50°	
70° L F	0°	70° L F	10° d	0°	
50° R F	45° d	15° R F	50° d	35°	
70° R F	0°	50° R F	38° d	20°	

Average displacement toward axis of stronger ear, 20.3°

ticking of a watch was determined for each ear both before and after the observations, and the ratio of sensitivity was computed from these distances. For observer *F.* this distance was found to be 40 cm. for each ear. Estimated in terms of these distances, then, the observer's ears were approximately equal in sensitivity. As the tables show, the ears of observers *R.* and *S.* were found to be of unequal sensitivity. It will be noted that a rough correspondence holds in each case between the ratio of sensitivity of the ears, and the observer's characteristic localizing tendency.

TABLE III

Observer *S.* Showing the influence of the binaural ratio on the localization of clangs. Natural sensitivity series. Liminal distance: right ear, 97 cm.; left ear, 33 cm. Ratio, Right : Left = 2.9. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
135° R F	0°	5° R F	5° d		40°
45° L F	0°	30° L F	0°	15°	
150° L F	45° d	10° L F	0°	20°	
0° B	45° u	0° B	45° u	0°	0°
180° F	45° d	5° L F	35° d		5°
45° R F	45° u	50° R F	35° u	5°	
135° R F	45° u	45° R F	52° u	0°	0°
160° L F	0°	0°	0°	20°	
0° B	90° u	30° R F	12° u	30°	
70° L F	0°	65° L F	0°	5°	
135° L F	0°	25° L F	0°	20°	
45° R F	45° d	10° R F	15° d		35°
0° B	90° u	25° R F	12° u	25°	
60° R F	0°	70° R F	18° u	10°	
150° R F	0°	50° R F	15° u	20°	
45° L F	45° u	0° B	2° d	45°	
60° L F	45° u	5° L F	30° u	55°	
160° R F	0°	25° R F	10° u	5°	
60° L F	45° d	40° L F	10° d	20°	
0° B	0°	0° B	5° d	0°	0°

Average displacement toward axis of stronger ear, 10.7°.

Tables IV and V show the effect upon localization for observers *R.* and *S.*, produced by exaggerating the natural ratio of sensitivity of the two ears by plugging the weaker ear. It will be noticed that in each case the observer's tendency to displace the sound toward the axis on the side of the stronger ear is increased. For example, with the natural difference in sensitivity, Observer *R.*'s average displacement toward the axis on the side of the stronger ear was 20.3° (Table II); with the exaggeration of the natural difference, the average displacement became 31.6° (Table IV). For Observer *S.*, the average displacement toward the axis on the side of the stronger ear, with the natural difference in sensitivity, was 10.7° (Table III); with the exaggeration of this difference, the displacement was increased to 17.2° (Table V).

Tables VI and VII show the effect upon localization, for Observers *R.* and *S.*, produced by plugging the stronger ear until it became less sensitive than the weaker ear. The result in each case was to change the characteristic displacement to the opposite side—now the side of the stronger ear. For

example, for Observer *R.*, when the ratio was changed from left: right=4, to right: left = 6.2, the average displacement of 20.3° toward the axis on the side of the left ear was changed to an average displacement on the side of the right ear of 34.4°. And for Observer *S.*, when the ratio right: left = 2.9 was changed to left: right = 3.8, the average displacement was changed from 10.7° toward the right to 34.6° toward the left.

TABLE IV

Observer *R.* Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 3 cm.; left ear, 71 cm. Ratio, Left : Right = 23.6. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	30° L F	40° d	30°	
45° R F	0°	10° L F	35° d	55°	
180° F	0°	60° L F	10° d	60°	
135° L F	0°	55° L F	15° d	10°	
60° L F	0°	70° L F	15° d	10°	
0° B	45° u	40° L F	48° d	40°	
135° R F	0°	45° R F	40° d	0°	0°
180° F	45° u	40° L F	50° d	40°	
45° L F	45° d	55° L F	12° d	10°	
135° R F	45° d	40° R F	43° d	5°	
0° B	45° d	35° L F	25° d	35°	
180° F	45° d	30° L F	15° d	30°	
45° R F	45° u	25° L F	45° d	70°	
135° L F	45° u	50° L F	15° d	5°	
45° L F	45° u	60° L F	40° d	15°	
135° R F	45° u	15° L F	32° d	60°	
0° B	90° u	15° L F	15° d	15°	
45° R F	45° d	35° L F	18° d	80°	

Average displacement toward axis of stronger ear, 31.6°.

Tables VIII and IX show the effect upon localization made by producing artificial differences in the sensitivity of the ears for Observer *F.*, whose natural sensitivity is approximately equal for both ears. When the left ear was plugged until the ratio right: left = 4.3 was obtained, the average displacement toward the axis on the side of the right ear was found to be 17°. And when the right ear was plugged until the ratio left: right = 4.1 was obtained, the average displacement toward the side of the left ear was found to be 18.2°. It will be remembered from Table I that the average of the localizations for this observer with natural hearing showed a displacement toward the right of 2.1°.

TABLE V

Observer *S.* Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 97 cm.; left ear, 10 cm. Ratio, Right : Left = 9.7. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	45° R F	0°	45°	
45° R F	0°	35° R F	0°		10°
135° L F	0°	5° L F	7° d	40°	
180° F	0°	10° R F	8° d	10°	
135° R F	0°	20° R F	0°		25°
45° L F	45° u	5° L F	14° d	40°	
0° B	45° u	50° R F	15° u	50°	
135° R F	45° u	40° R F	8° u		5°
135° L F	45° u	25° L F	12° u	20°	
180° F	45° u	30° R F	20° u	30°	
45° R F	45° u	52° R F	30° u	7°	
45° L F	45° d	35° L F	8° d	10°	
0° B	45° d	20° R F	8° u	20°	
45° R F	45° d	60° R F	15° u	15°	
135° L F	45° d	45° L F	30° u	0°	0°
180° F	45° d	20° R F	15° u	20°	
135° R F	45° d	45° R F	10° u	0°	0°
0° B	90° u	40° R F	10° u	40°	
60° L F	0°	40° L F	10° u	20°	

Average displacement toward axis of stronger ear, 17.2°.

Table X shows the effect upon localization, in the case of Observer *R.*, of an attempt to equate the sensitivity of the ears. The natural ratio of sensitivity for this observer (Table II) was left:right = 4. When this was changed by plugging the left ear until the ratio right:left = 1.02 (18.5÷18.2) was obtained, there resulted an average displacement 31.4° toward the axis on the side of the right ear. Results of this kind were obtained for all observers. A characteristic tendency to displace the sound to right or left cannot be corrected by equating the ratio of sensitivity of the ears. A value must be obtained somewhere between the natural ratio and equal sensitivity.[1]

[1]That the effect of equating the sensitivity should overshoot the mark is not at all strange. If one ear naturally hears more loudly than the other, an equal intensity of sensation has never been associated in the observer's experience with objects in the median plane, but always with some position displaced from the median plane toward the naturally weaker ear. Then when the stronger ear is plugged until it is of the same sensitivity as the weaker ear, sounds which can be heard as equally loud by both ears, *i. e.*, sounds coming from the median plane, will not be referred to that plane,

TABLE VI

Observer *R*. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 18.5 cm.; left ear, 3 cm. Ratio, Right : Left = 6.2. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	65° R F	0°	20°	
0° B	90° u	15° R F	42° d	15°	
180° F	45° d	25° R F	40° d	25°	
60° L F	0°	25° R F	40° d	85°	
135° R F	0°	40° R F	25° d		5°
45° R F	45° d	45° R F	18° d	0°	0°
135° L F	45° d	4° R F	40° d	49°	
0° B	45° d	30° R F	32° d	30°	
45° L F	45° d	55° R F	20° d	100°	
135° L F	45° u	5° R F	35° d	50°	
135° R F	0°	40° R F	20° d		5°
45° R F	45° u	50° R F	10° d	5°	
180° F	45° u	25° R F	38° d	25°	
45° L F	45° u	45° R F	42° d	90°	
135° R F	45° u	45° R F	45° d	0°	0°
0° B	45° u	45° R F	20° d	45°	
180° F	0°	25° R F	38° d	25°	
135° L F	0°	5° R F	28° d	50°	
0° B	0°	50° R F	40° d	50°	

Average displacement toward axis of stronger ear, 34.4°.

Tables XI and XII show the results of fairly successful attempts to correct this observer's tendency to displace sounds towards the left. In Table XII with a ratio left:right = 1.5 we find an average displacement of 2.1° towards the right; in Table XI with a ratio left:right = 2.1 we find an average displacement of 1.4° towards the left. The former tables thus show over-correction; the latter, under-correction.

B. THE RELATIVE IMPORTANCE OF INTENSITY AND TIMBRE AS FACTORS IN LOCALIZATION

In these experiments, tuning forks were used as the source of sound. The object was to find out to what extent the con-

but will be displaced toward the axis on the side of the weaker ear, because sounds of equal intensity have always had that connotation in the observer's past experience. Likewise, when the ears have been made equally sensitive by plugging the stronger, sounds which come from positions to either side of the median plane will always be displaced toward the naturally weaker ear, because now they are heard by the two ears with a relative loudness which, in the observer's past experience, has always connoted a position relatively nearer the weaker ear.

TABLE VII

Observer *S*. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 18 cm.; left ear, 70 cm. Ratio, Left : Right = 3.8. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	30° L F	5° d	30°	
180° F	0°	30° L F	20° d	30°	
45° R F	0°	10° L F	5° d	55°	
45° L F	45° u	60° L F	5° u	15°	
0° B	45° u	33° L F	22° d	33°	
135° R F	0°	20° L F	5° d	65°	
135° L F	0°	50° L F	15° d	5°	
180° F	45° u	30° L F	0°	30°	
135° R F	45° u	20° L F	18° u	65°	
135° L F	45° u	65° L F	5° u	20°	
0° B	45° d	25° L F	0°	25°	
45° R F	45° u	35° L F	10° u	80°	
45° L F	45° d	50° L F	15° u	5°	
60° L F	0°	6° L F	5° u		54°
45° R F	45° d	30° L F	12° d	75°	
180° F	45° d	40° L F	12° d	40°	
135° R F	45° d	30° L F	5° d	75°	
0° B	90° u	30° L F	5° d	30°	

Average displacement toward axis of stronger ear, 34.6°.

ditions obtaining in the former experiments influence the localization of simple tones. Three cases are possible. (a) These conditions may exert no influence at all. We should then have to conclude that, in the former experiments, the binaural ratio produces its effect wholly as a difference in the timbre of the sound as heard by the two ears. That is, since timbre depends upon the number and the proportionate strength of the overtones in the clang, in case one ear is more sensitive than the other, the timbre of the sound heard by one ear will differ from that heard by the other ear because of the different number of overtones present in the two cases.[1] (b) The conditions may exert some influence, but not

[1]This view of the way the binaural ratio serves as a localizing clue was first advanced by Rayleigh in 1876, and later by Sylvanus Thompson (*Op. cit.*, p. 415) in 1882. Thompson says: "Judgments as to the direction of sound are based, in general, upon the sensations of different intensity in the two ears, but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity in the lowest or fundamental tone of the compound (or 'clang'), but upon the difference in intensity of the individual tone or tones of the clang for which the intensity difference has the greatest effective result in the quality of the sound . . . . . It is completely open to doubt whether a pure simple tone heard in one ear could suggest any direction at all."

as much as was exerted upon the sound of the Galton whistle. In this case, we should have to conclude that differences of intensity both in the fundamental and in the overtones of the clang served as a localizing clue in our experiments. (c) They may exert an equal influence upon the sound of the tuning fork and upon the clang. This would indicate that differences in the intensity of the fundamental tone alone were operative as local signature.

TABLE VIII.

Observer *F*. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Left ear plugged. Liminal distance: right ear, 39 cm.; left ear, 8 cm. Ratio, Right : Left = 4.9. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	70° R F	8° u	30°	
135° L F	0°	35° L F	0°	10°	
0° B	0°	35° R F	0°	35°	
135° R F	0°	50° R F	0°	5°	
45° L F	45° u	50° L F	0°		5°
180° F	0°	0° B	0°	0°	0°
135° L F	45° u	45° L F	30° d	0°	0°
45° L F	45° d	0° B	30° d	45°	
150° R F	0°	30° R F	5° d	0°	0°
0° B	45° u	10° R F	10° d	10°	
45° R F	45° u	45° R F	20° u	0°	0°
180° F	45° u	155° R F	10° u	25°	
135° R F	45° u	170° R F	20° u		35°
150° L F	45° u	35° L F	0°		5°
0° B	90° u	25° R F	0°	25°	
45° L F	0°	0° B	20° d	45°	
0° B	45° d	40° R F	10° d	40°	
180° F	0°	15° R F	10° u	15°	

Average displacement toward axis of stronger ear, 17°.

The stimulus was given as follows in these experiments. The observer, blindfolded, with head firmly clamped and ears tightly closed, sat in position in the sound-cage. A heavy unmounted tuning fork of 480 vibrations per second and a cylindrical resonator were used as the source of sound. These were substituted for the telephone receiver of the sound-cage. The fork was plucked by the fingers covered by a chamois glove, and was allowed to vibrate for a few seconds to allow possible high overtones, harmonic or inharmonic, to die out. It was then held over the mouth of the resonator.

As soon as the tone became audible, the observer's ears were uncovered and the sound was listened to for about one second, at the end of which time the fork was removed from the mouth of the resonator, and the direction in which the sound was heard was indicated by the observer. In no case were any of the noises attendant upon the stimulation of the fork heard; and a tone as simple as a tuning fork is capable of giving was obtained. The duration of the stimulus was roughly the same

TABLE IX

Observer *F*. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Right ear plugged. Liminal distance: right ear, 10 cm.; left ear, 41 cm. Ratio, Left: Right = 4.1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
Horizontal	Vertical	Horizontal	Vertical		
45° R F	0°	40° R F	0°	5°	
135° L F	0°	110° L F	0°	25°	
0° B	0°	15° L F	10° u	15°	
135° R F	0°	15° R F	20° u	30°	
45° L F	45° u	60° L F	5° u	15°	
180° F	0°	50° L F	25° u	50°	
135° L F	45° u	90° L F	35° u	45°	
45° L F	45° d	55° L F	10° d	10°	
150° R F	0°	120° R F	18° u		30°
0° B	45° u	25° L F	30° u	25°	
45° R F	45° u	25° R F	30° u	20°	
180° F	45° u	35° L F	32° u	35°	
135° R F	45° u	15° R F	35° u	30°	
150° L F	45° u	90° L F	5° u	60°	
0° B	90° u	5° R F	20° u		5°
70° L F	0°	75° L F	0°	5°	
50° R F	45° d	120° R F	15° u		10°
70° R F	0°	30° R F	15° u	40°	

Average displacement toward axis of stronger ear, 18.2°.

as that of the Galton whistle used in the earlier experiments, and care was taken to give the stimulus as nearly as possible the same intensity each time. The stimuli were all given at the level of the ears, and no account of vertical displacements was taken in recording the results, since these have no direct bearing upon the purpose of the experiment.

Tables XIII, XIV, and XV give the results of this investigation. These results, on the average, show that the ratio of sensitivity of the two ears affects the localization of simple tones almost, if not quite, as much as it does the localization of clangs of the degree of complexity of the Galton whistle.

TABLE X

Observer *R*. Showing the influence of the binaural ratio on the localization of clangs. Sensitivity of two ears equated. Left ear plugged. Liminal distance: right ear, 18.5; left ear, 18.2 cm.; Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	50° R F	42° d	50°	
180° F	0°	35° R F	30° d	35°	
0° B	45° u	40° R F	30° d	40°	
180° F	45° u	50° R F	8° d	50°	
0° B	45° d	40° R F	25° d	40°	
180° F	45° d	30° R F	25° d	30°	
0° B	90° u	30° R F	42° d	30°	
0° B	0°	35° R F	40° d	35°	
180° F	0°	30° R F	44° d	30°	
0° B	45° u	20° R F	28° d	20°	
180° F	45° u	20° R F	32° d	20°	
0° B	45° d	10° R F	35° d	10°	
180° F	45° d	35° R F	35° d	35°	
0° B	90° u	15° R F	38° d	15°	

Average displacement toward axis of right ear, 31.4°.

TABLE XI

Observer *R*. Showing the influence of the binaural ratio on the localization of clangs. Attempt to correct localizing error. Left ear plugged. Liminal distance: right ear, 19 cm.; left ear, 40 cm. Ratio, Left : Right = 2.1. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	10° R F	28° d	10°	
180° F	0°	20° L F	35° d		20°
0° B	45° u	0° B	32° d	0°	0°
180° F	45° u	10° L F	25° d		10°
0° B	45° d	0° B	35° d	0°	0°
180° F	45° d	5° R F	35° d	5°	
0° B	90° u	5° R F	50° d	5°	
0° B	0°	0° B	32° d	0°	0°
180° F	0°	0° B	20° d	0°	0°
0° B	45° u	10° L F	35° d		10°
180° F	45° u	5° L F	30° d		5°
0° B	45° d	15° R F	32° d	15°	
180° F	45° d	0° B	20° d	0°	
0° B	90° u	10° L F	42° d		10°

Average displacement from median plane, 1.4° (left).

TABLE XII

Observer *R*. Showing the influence of the binaural ratio on the localization of clangs. Artificial sensitivity series. Attempt to correct localizing error. Left ear plugged. Liminal distance: right ear, 19 cm.; left ear, 29 cm. Ratio, Left: Right = 1.5. Stimulus, Galton whistle, 20,000 vibrations per second.

Set		Heard		Displacement toward axis of right ear	Displacement toward axis of left ear
Horizontal	Vertical	Horizontal	Vertical		
0° B	0°	5° R F	30° d	5°	
180° F	0°	5° L F	15° d		5°
0° B	45° u	10° R F	30° d	10°	
180° F	45° u	0° B	30° d	0°	0°
0° B	45° d	0° B	15° d	0°	0°
180° F	45° d	0° B	15° d	0°	0°
0° B	90° u	0° B	40° d	0°	0°
0° B	0°	5° R F	38° d	5°	
180° F	0°	5° L F	22° d		5°
0° B	45° u	15° R F	30° d	15°	
180° F	45° u	0° B	35° d	0°	0°
0° B	45° d	0° B	25° d	0°	0°
180° F	45° d	0° B	25° d	0°	0°
0° B	90° u	5° R F	40° d	5°	

Average displacement from median plane, 2.1° (right).

But the effect is not nearly so consistent in the individual judgments. When the Galton whistle was used as stimulus, the sound was displaced toward the axis on the side of the stronger ear in a very large percentage of cases, and, relatively speaking, in not widely varying amounts. In the case of the tuning fork, however, a very large displacement of the sound toward the axis on the side of the stronger ear was frequently followed by one toward the axis on the side of the weaker ear, the variation in the individual judgments from the true position being, in general, very much greater than for the Galton whistle. It would appear then, in these experiments, that the binaural ratio has exerted its influence both as difference in intensity and as change of timbre.[1] For the sake of comparison,

[1]A few words, further explaining and qualifying the above argument, are probably not out of place here. The tone of the Galton whistle set at 20,000 vibrations is relatively simple. The first overtone, for example, has a vibration rate of 40,000, and the second of 60,000, which is above the limit of audibility. Thus our argument that the above mentioned displacements have been made in terms of the intensity factor should not rest so much, probably, upon a correspondence of results when Galton whistle and tuning fork are used as sources of sound, as upon the fact that the large displacements observed took place both when a simple and a relatively simple tone were used as stimuli. To complete the investigation a comparison should

TABLE XIII

Observer *F*. Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: right ear, 40 cm.; left ear, 40 cm. Ratio, Right : Left = 1. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	Displacement toward axis of right ear	Displacement toward axis of left ear
135° L F	70° L F		25°
0° B	0° B	0°	0°
45° L F	60° L F		15°
135° R F	45° L F		90°
60° L F	60° L F	0°	0°
120° L F	120° L F	0°	0°
50° L F	80° L F		30°
180° F	0° B	0°	0°
150° L F	65° L F		35°
0° B	0° B	0°	0°
135° L F	90° R F	135°	
45° R F	45° R F	0°	0°
0° B	125° R F	55°	
60° R F	15° R F		45°
180° F	85° L F		85°
120° R F	60° R F	0°	0°
30° R F	130° R F	20°	
150° R F	135° R F	15°	
0° B	10° R F	10°	
45° R F	90° R F	45°	
180° F	20° L F		20°

Average displacement from median plane, 3.09° (left).

the same observers were used here that were used in the experiments with the Galton whistle. In order to show the effect of the binaural ratio, it was deemed advantageous, in both cases, to work with observers both of equal and of unequal sensitivity of the two ears.

Table XIV shows the results for Observer *R*. The ratio of sensitivity was chosen so that left: right = 2.3; the average displacement toward the axis on the side of the stronger ear was found to be 15°. A correlation of average displacement with ratio of sensitivity shows, roughly speaking, for this observer, quite as much tendency to displace the sound to the side of the stronger ear as was shown for the Galton whistle.

Results with the Galton whistle are brought forward from Tables II, IV, and VI for comparison. Table II shows for *R*.

be made further of the results obtained with these two sources of sound and one still more complex than the Galton whistle. This comparison will be included in the work on this problem still in progress in this laboratory.

a ratio of sensitivity left: right = 4, an average displacement toward left of 20.3°; Table IV, a ratio of sensitivity left: right = 23.6, an average displacement toward the left of 31.6°; Table VI, a ratio of sensitivity right: left = 6.2, and an average displacement of 34.4° toward the right.

Table XV shows the results for Observer *S.*, with a ratio of sensitivity chosen so that right: left = 1.9. With this ratio it was found that the sound was displaced, on the average, 7.5° toward the side of the stronger ear. When compared with a ratio left: right = 2.9 and an average displacement of 10.7° toward the left ear (Table III), a ratio right: left = 9.7 and an average displacement toward the right of 17.2° (Table V), and a ratio left: right = 3.8 with an average displacement toward the left of 34.6° (Table VII), these results also show probably as strong a tendency to displace the simple tone toward the stronger ear as was shown in the case of the clang.

TABLE XIV

Observer *R.* Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: left ear, 49 cm.; right ear, 21 cm. Ratio, Left : Right = 2.3. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	displacement toward axis of stronger ear	Displacement toward axis of weaker ear
45° R F	85° R F		4°
135° R F	30° R F	15°	
0° B	40° L F	40°	
60° R F	40° R F	20°	
180° F	0° B	0°	
30° R F	55° R F		25°
150° R F	40° L F	70°	
0° B	55° R F		55°
45° R F	55° R F		10°
135° R F	50° L F	95°	
0° B	85° L F	85°	
135° L F	35° L F		10°
0° B	35° R F		35°
45° L F	75° L F	30°	
0° B	50° L F	50°	
120° L F	55° L F		5°
50° L F	55° L F	5°	
180° F	60° L F	60°	
150° L F	40° R F		70°
0° B	45° L F	45°	
135° L F	70° L F	25°	
45° L F	85° L F	40°	

Average displacement toward axis of stronger ear, 15°.

TABLE XV

Observer *S.* Showing the influence of the binaural ratio on the localization of simple tones. Liminal distance: right ear, 86 cm.; left ear, 46 cm. Ratio, Right : Left = 1.9. Stimulus, tuning fork, 480 vibrations per second.

Set	Heard	Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
135° L F	35° R F	80°	
0° B	25° L F		25°
45° L F	35° L F	10°	
0° B	40° L F		40°
60° L F	70° L F		10°
120° L F	40° R F	100°	
50° L F	65° L F		15°
180° F	45° R F	45°	
135° R F	0° B		45°
150° L F	10° L F	20°	
0° B	50° R F	50°	
180° F	10° L F		10°
135° L F	120° R F	105°	
45° L F	75° L F		30°
45° R F	10° R F	25°	
0° B	70° L F		70°
60° R F	75° R F	15°	
0° B	15° L F		15°
180° F	50° R F	50°	
120° R F	45° R F		15°
30° R F	60° R F	30°	
45° R F	45° L F		90°

Average displacement toward axis of stronger ear, 7.5°

C. INDIVIDUAL PREFERENCES

Of the individual preferences reported by von Kries[1] and Dunlap,[2] the writers find this much evidence. There is (1) a

[1]The individual preferences mentioned by von Kries (*Ueber das Erkennen der Schallrichtung*, Ztschr. f. Psychol., I, 1890, s. 242-243) are confined to points in the median plane. The results obtained by us bear more specifically upon the preferences reported by Dunlap.

[2]Dunlap (*The Localization of Sounds*, Psychol. Rev. Monog. Suppl., 1909, Vol. X, No. 40, 1-16) says: "Several years ago I commenced the attempt to make comparisons between the location of sounds with both ears and the location with one ear, the other being stopped as well as might be. The results of my first tests were rather odd, showing a condition which made it impossible to get at the comparisons I wished, at least in any clear way; and subsequent tests which I have made from time to time, and which students have made for me, on different subjects, have resulted in the same way. The condition mentioned has had so little (if any) consideration in connection with the problem of the location of sounds, that I have thought it important to give some account of my experiments." The condition

tendency in case of a stronger ear, to refer the sound in the direction of this ear; and (2) in the case of his observers, and the relatively weak stimulus used, there seemed to be a fairly consistent tendency to prefer the back to the front locations; in fact, some observers never located a sound in front. The former tendency gave certain observers a decided right or left "preference," depending upon their defect; and this, combined with the second tendency, tended to limit the localizations to a single quadrant. That is, the back tendency operating to limit the sound to one hemisphere, and the right or left tendency to a hemisphere at right angles to this, would tend to confine the localizations to one quadrant for a given observer. But these tendencies can hardly be called capricious, as Dunlap apparently found them to be. To show that the one conforms to law, *i. e.*, is correlated with a definite sensory characteristic, has been the object of this paper. The other is still under investigation.[1]

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which discouraged further work on his problem may be summed up in his own words: "The position in the area of location bears precious little relation to the actual position of the sound. The marks representing the sounds at the various points might to all intents and purposes be shaken up in a box and dumped down on the preferred area on the chart. This appearance is amply confirmed by other series on the subjects. Repeated series give results which have no uniformity, except in the general area of location.

"The preferred position is not determined by the character of the sound or by the environment. Two subjects in exactly the same circumstances may have quite different preferences. A subject may show the same preference after six months or a year, or may show a decidedly different one, without any known reason for the change. The subject shows the same preference in different rooms, or if he is reversed in the same room. Alterations in the intensity of the sound produced no definite alterations in the preferred area. The Galton whistle gives practically the same results as the buzzer or the telephone receiver. So far I have not found a subject who does not localize in this preferential way. What the causes are, I cannot say. There are possible theories and nothing more. Meantime, how to conduct profitable experiments in localization before solving this problem is another problem."

The writers admit that the irregularity of Dunlap's results is discouraging. After a careful study of Dunlap's charts of results, they also admit that the factors underlying the evidences of individual preferences of their own observers fail utterly to solve "the puzzle." They can only repeat that in their contention for a lawful mechanism, they do not wish to go beyond the results and conditions of their own experiments; and merely suggest that Dunlap may have worked with the subjective type of observer, and may have fostered this subjective tendency by the use of the chart method for indicating directions.

[1]It has been discussed at various times in the literature of auditory localization whether back reference may not have become associated with weak intensities of sound. (Thompson: *The Pseudophone*, Phil. Mag. (5) VIII, 1879, 385-390; Bloch: *Das binaurale Hören*. Wiesbaden, 1893, pp. 52-6; Pierce: *Op. cit.* 90-1. A reason for this association has been found in the shape of the external ear as a collector of sound (Pierce: *Op. cit.*, p. 90; Bloch: *Op. cit.*, pp. 25-52). But apart from the cause the writers are investigating the fact in the following way. Observers are selected who,

Moreover, no variation that could be called a consistent change occurred in these tendencies after a lapse of some four months. Certainly nothing has come out with regard to the right or left tendency, from time to time, that cannot be roughly correlated with the results of the accompanying sensitivity tests, as will be shown by the results given in the next section of this paper.

However, in contending for a lawful mechanism, and in suggesting an explanation, of what, on the surface, might be considered as capricious, the writer has no desire to go beyond the results and conditions of his own experiments. Each case must be tried out on its own merits.

Table XVI shows the preference for the back locations. This table was compiled from the results of Tables I-XV inclusive and from Table XVII. The number of readings given back and in front of the aural axis, the number of times the sound was localized in the correct hemisphere and the number of times it was displaced to the opposite hemisphere was determined from the tables; and the ratio of the number of backward displacements to the number of forward displacements was computed from these results.

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with the comparatively weak stimulus used in the preceding experiments, show a marked tendency to locate the sound behind. A graded series of stimuli is then provided, ranging from very weak to very strong, and the regular localizing series is given for each stimulus. If, with an increase in the intensity of the stimulus, there is found to be a decrease in the percentage of back references, the intensity of the stimulus may fairly be said to sustain an associative relation to this direction reference. This method of procedure, the writers believe, offers better possibilities of getting results from which conclusions can be drawn than do the experiments of the kind performed by Bloch, even though Bloch's principle of working be carried out under laboratory conditions. Bloch selected a sound of such intensity that when it was given behind, it was localized behind by his observers; and then he tried the effect of an increase of intensity. He claimed thus to be able to cause a reversal of the localization, or the illusion of front. Bloch's method of experimenting was extremely crude. The experiments were made in the open air in a court enclosed on three sides. "The observer stood 5 meters from the end wall and pebbles were thrown on the stone pavement in front or behind him. The result was that when his face was turned towards the wall, the legitimate influence of the pinna was merely increased and the localizations were mostly correct. When, on the contrary, the back was turned towards the wall, sounds coming from behind were apt to be falsely located in front, since now the reflection of the sound waves by the wall produced an unwonted intensity in the sound." Applying this principle of working under laboratory conditions in various ways, the writers have always failed to get anything like consistent results. Any attempt to confirm the association of the back or front reference with the intensity of the sound, based upon individual judgments, they believe is doomed to failure. If conclusions are to be reached at all they must be reached from a comparison of averages got by a systematic variation of intensity.

TABLE XVI

Showing, with the comparatively weak stimulus used, the preference of our observers for back locations.

Tables from which data are taken	No. of readings given back of aural axis	No. localized in correct hemisphere	No. displaced in front of aural axis	No. of readings given in front of aural axis	No. localized in correct hemisphere	No. displaced back of aural axis	Ratio of displacement back to displacement front
Table I	11	10	1	8	3	5	5 : 1
Table II	10	10	0	8	0	8	8 : 0
Table III	12	12	0	8	0	8	8 : 0
Table IV	10	10	0	8	0	8	8 : 0
Table V	10	10	0	9	0	9	9 : 0
Table VI	10	10	0	9	0	9	9 : 0
Table VII	10	10	0	8	0	8	8 : 0
Table VIII	9	9	0	9	2	7	7 : 0
Table IX	10	9	1	8	4	4	4 : 1
Table X	8	8	0	6	0	6	6 : 0
Table XI	8	8	0	6	0	6	6 : 0
Table XII	8	8	0	6	0	6	6 : 0
Table XIII	11	9	2	10	3	7	7 : 2
Table XIV	13	13	0	9	0	9	9 : 0
Table XV	13	13	0	9	1	8	8 : 0
Table XVII	9	9	0	8	0	8	8 : 0

D. THE QUESTION OF CHANGES IN THESE PREFERENCES WITH LAPSE OF TIME

Experiments were conducted to find out whether any considerable change occurred in the observer's tendency to localize during the course of several months, or, more especially, to determine whether there occurred any change that could not be correlated with a corresponding change in the ratio of sensitivity of the ears. No change of any significance was found to have taken place in any of the cases examined. Table XVII shows the results obtained for observer *R.* three months later than those given in Table II. These results may be taken as representative. In Table II, the ratio left : right = 4, and the displacement is 20.3° toward the left; and in the table given below, the ratio left : right = 3.7, and the displacement is 23.3°. This comparison shows a slight increase in the observer's tendency to refer the sound to the side of the stronger ear, but in a field where the results show such a large mean variation the writers have not con-

sidered, either here or elsewhere in the work, that so small a change in results is at all significant.

By comparing the results of Table XVII with Tables II, VI, VII, X, XI, XII, and XIV, in Table XVI, it will be found also that no significant change has occurred in the observer's preference for the back locations.

TABLE XVII

Observer *R*. Showing that no change of any consequence has taken place in the localizing tendency of our observers after a lapse of three months. (Compare with Table II.) Liminal distance: right ear, 19 cm.; left ear, 71 cm. Ratio, Left : Right = 3.7. Stimulus, Galton whistle, 20,000 vibrations per second.

Set	Heard	Displacement toward axis of stronger ear	Displacement toward axis of weaker ear
0° B	25° L F	25°	
180° F	50° L F	50°	
45° L F	65° L F	20°	
130° L F	65° L F	15°	
70° L F	70° L F	0°	0°
135° L F	80° L F	35°	
0° B	15° L F	15°	
135° R F	15° R F	30°	
45° R F	25° R F	20°	
150° R F	20° R F	10°	
60° L F	75° L F	15°	
135° R F	10° R F	35°	
60° R F	10° R F	50°	
50° L F	60° L F	10°	
130° R F	25° R F	25°	
180° F	20° L F	20°	
0° B	20° L F	20°	

Average displacement toward axis of stronger ear, 23.2°.

III. SUMMARY OF RESULTS.

(1) Subjects having a natural difference in the sensitivity of the two ears show a constant tendency to displace the sound toward the axis on the side of the stronger ear; and, conversely, subjects without this difference in sensitivity do not show this tendency. The greater number of subjects examined showed a difference in sensitivity.

(2) Changes in the ratio of sensitivity, produced by plugging either ear, were followed by corresponding displacements of the sound toward the axis on the side of the stronger ear. Differences in sensitivity, artificially produced, apparently

exerted a greater influence upon localization than did approximately equal differences due to natural defect. This is probably because, in the case of a natural defect, the localization error has been partly corrected, in the past experience of the subject, through association with the direction reference of other sense-organs.

(3) In the case of observers who showed a characteristic right or left tendency, it was found possible to change the ratio of sensitivity so that the error in localization was corrected. This result was not accomplished by equating the sensitivity of the two ears. The desired ratio was always found to have a value somewhere between equal sensitivity and the old ratio.

(4) The average results showed that changes in the binaural ratio affected the localization of simple tones almost, if not quite, as much as it did the localization of clangs of the degree of complexity of the Galton whistle. The individual judgments, however, showed a much larger variation from the true position in the case of the simple tones. It would appear, then, that in these experiments the binaural ratio exerted its influence both as difference in intensity and as change of timbre, but predominantly as difference in intensity.

(5) The writers find this much evidence of individual preferences in localization (von Kries', Dunlap). (*a*) There is a tendency in case of a stronger ear to refer the sound in the direction of that ear. This gave certain observers a decided right or left tendency, depending upon the kind and amount of their defect. (*b*) With the relatively weak stimulus used, there seemed to be a fairly constant tendency for the observers to prefer back to front locations. But these tendencies cannot in any sense be called capricious. One is directly traceable to the binaural ratio; the other is still under investigation, and is probably an effect of the intensity of the stimulus used.

(6) No changes of any consequence in these tendencies were found during the course of several months, as occurred in the case of Dunlap's observers,—certainly none that could not be correlated with a definite change in the localizing clue. For example, a cold, or what not, was sometimes found to produce a change in the observer's right or left tendency, but tests of the sensitivity of the two ears always disclosed a corresponding change in the binaural ratio.

In this study, nothing was undertaken bearing upon the later aspects of the intensity theory brought out by the papers of Rayleigh[1] and Wilson and Myers.[2] The writers, however,

[1]Rayleigh: *On our Perception of Sound Direction*, Philos. Mag. (6), XIII, 1907, pp. 214-32.
[2]*Op. cit.*

have begun experiments upon three points relative to these aspects. (1) It will be determined whether tones of 128 vibrations or less per second have a larger j. n. d. of direction than tones of higher pitch. This the intensity theory would seem to require, according to Rayleigh's calculations of the relative intensity of the waves received by the two ears. Rayleigh's tests of this point were as rough as possible. They consisted, it will be remembered, in determining whether stimuli of both low and high pitch, given in the region of the aural axis, could be judged as right or left without mistake. Now, working under these conditions, a considerable difference in direction-sensitivity might obtain for the two kinds of tones, and still no mistake be made in either case. The positions chosen give the largest possible binaural ratio, and the judgments required are the most general that could possibly be made. In short, a less sensitive method for detecting small differences in power to discriminate direction could hardly have been devised. The size of the j. n. d. is obviously the proper criterion to apply. (2) The series of experiments used in this paper will be repeated, using forks of low and high frequency. If there is found as much tendency to displace the tones of low pitch as those of high pitch, the results should argue that the localizing clue for low tones is the binaural ratio, instead of the power directly to detect phase differences; because (a) the change in the sensitivity of the ear does not affect the phase of the sound wave, and (b) it could not affect the detection of the phase differences by the ear in such a manner as to displace the sound toward the stronger ear, for by this hypothesis, ratio of effect has nothing to do with localization. At least, it cannot be assumed that the binaural ratio, which is computed in terms of intensive difference, could be translated directly into terms of recognition of phase difference. Apparently the only effect that could follow a decrease of sensitivity of one ear would be a proportionate confusion and uncertainty of localization, not a definite and characteristic displacement toward the axis on the side of the stronger ear. (3) The settings given to the stimuli in Wilson and Myers's experiments will be repeated under ordinary localizing conditions, in order to see whether the transfer of the sound from one side of the median plane to the other takes place when the direct paths of transmission to the two ears are changed by the amounts they used. If the transfers do not take place, some evidence, at least, will be afforded that the experiments they describe and the conclusions they reach do not bear directly upon the phenomenon of localization as it ordinarily occurs, but only upon a special

phenomenon created by their conditions, which favored bone conduction.

The writers present this report with the hope that their results establish a more definite correlation between the binaural ratio and direction-reference than has previously been attained, and that the experiments described will provide an easily available means of clearly demonstrating this correlation in the teaching laboratory.

A REPLY TO PROFESSOR SAFFORD

By F. M. URBAN, University of Pennsylvania

Professor Safford published in the January number of this *Journal* a little note containing a criticism of my theory of psychophysical measurements. His objections are two in number. The first refers to the number of decimals which have been retained in my tables; he is of the opinion that the computation should have been carried to the third decimal only rather than to the fourth. The second objection is of a more complicated nature, and refers to my use of Lagrange's formula. Professor Safford's ideas are very interesting and I am glad to have an opportunity to explain some considerations, at which my original articles merely hint. My book would have become very voluminous, had I undertaken to present all the chains of reasoning which I later found to be wrong, or to describe all the considerations which decided me to adopt a certain manner of procedure. Both of Professor Safford's objections occurred to me in the course of working out my data, and I may be allowed to state the reasons why I believe that they are erroneous.

Before answering the first objection I want to say that in computing data of this sort it is customary to express one's results in four places of decimals. The majority of statistical investigations, however, do not use relative frequencies, as I did, but percentages calculated to the second decimal. Precentages are found from relative frequencies by multiplying by 100; two decimals in percentages, therefore, correspond to four decimals in relative frequencies. Saying that an event has the relative frequency 0.4422 is the same as saying that this event occurs in 44.22 per cent. of the cases. The latter form is, perhaps a little more familiar to the eye; but relative frequencies have the advantage over percentages of being the more primitive notions.

The relative frequencies of the different judgments form the starting-point of my exposition of the theory of psychophysical measurements; and I naturally gave much thought to the question of how many decimals should be retained. The mere physical labor of carrying out the computations—including the unavoidable wild goose chases—was very considerable indeed, and it seemed highly desirable not to increase the task by carrying too many decimals. Not being satisfied with the reference to custom and being acquainted with the theory of physical measurements, my mind naturally drifted into the channels pointed out by Professor Safford. I found out very soon that there does not exist a universal agreement as to the number of decimals which should be retained in the result, and that the rules explained by Professor Safford are not the only way of approaching the problem, for no less an authority than Gauss advocates the rule that the computations should be carried so far that the final result should enable one to calculate the actual data of observation with their original precision.

Professor Safford says that it is customary to use the average deviation as a measure of precision and to retain two significant figures of it. There does not exist any such general custom. In Germany and Austria the mean error is in almost exclusive use, while English, American and some French textbooks on the method of least squares recommend the probable error. Something may be said in favor of each one of these quantities, but this is not the

topic of the present discussion, where we only want to see whether there exists a universal agreement as to the quantity which is to be used as a measure of precision. Neither does there exist an agreement as to the number of significant figures to be retained in the measure of precision. I open the chapter on the adjustment of observations in Czuber's text-book of the calculus of probabilities and find on pages 293, 294 and 298 examples in which the mean error is calculated to one, two, three and four significant figures. Some of these examples are taken from authoritative sources, so that one cannot possibly say that there exists a general rule as to the number of decimals which should be retained in the result. Two or three significant figures in the measure of precision seem to be most frequently used.

There exists a fundamental difference between the data of statistical observation and the results of physical measurements, which Professor Safford entirely overlooks. The results of physical measurements are exact within one-half of the last significant figure. Thus if we put down 29 inches as the length of a line, this result means that the line is not longer than 29.5 nor shorter than 28.5 inches. When using this result in a computation, one must not add digits to it, because the following figures are entirely unknown. The case of statistical observation is different. If we observe that an event takes place 29 times in 100 cases, both these numbers are absolutely exact. The figure 29 does not indicate a result which may vary between 28.5 and 29.5; but it means exactly 29 and we may add as many zeros as seems necessary. The number of decimal places retained is merely a question of convenience, and one cannot be accused of publishing a misleading result if the accuracy of the determination accompanies it.

It took me some time to see that the theory of physical measurement is not the most direct way of determining the precision of my observations. The original data of my experiments are determinations of the probabilities of the different judgments. The most direct way of finding the precision of these observations is given by Bernoulli's theorem. This theorem refers to observations, in which a chance event A occurred n times in a total number of cases N; and it gives the most probable value of the unknown probability of this event and the limits of the accuracy of this determination. This is exactly the case of my experiments and I chose the probable error determined by Bernoulli's theorem as the measure of precision. A table of these probable errors is printed in the *Archiv f. d. ges. Psychologie*, 1909, Vol. 15, p. 287. The probable errors in the determination of the probabilities of the "greater" judgments for Subject I on the comparison stimuli 84, 88, 92, 96, 100, 104 and 108 were found to be 0.0015, 0.0044, 0.0090, 0.0132, 0.0157, 0.0097 and 0.0076. Admitting that two or three significant figures in the measure of precision is a conservative accuracy, I decided to retain four decimals in the tables of the relative frequencies of the judgments.

It was the traditional custom in statistics and psychophysics to use the methods of physical measurement uncritically. Lexis and his followers have shown how statistical data must be treated, and I tried to develop the theory of psychophysical measurement. That this can be done has been shown. At present we have a number of psychophysical methods which may stand alone on their own merit. Neither did I decide hastily, in breaking away from the old notions. It may be that there is a connection between the theories of physical and of psychophysical measurement; but at present we know only little about it, and the only statement which one could make with any kind of confidence is, that the theory of physical measurement must be based on that of psychophysical measurement. The so-called law of the distribution of errors of observation has resisted all attempts at a purely mathematical demonstration. Innumerable attempts have been made to explain this law—some of them by the cleverest mathematicians the world has known—but all have failed in so far as their proofs necessitated the introduction of some one assumption which is equivalent to the propo-

sition to be proved. It is only natural to suppose that this law contains some supposition of non-mathematical nature. It seems that it depends, in some way, on our method of making observations, and that the so-called Gaussian coefficient of precision is closely related to the threshold of difference, a fact which would justify Gauss in putting this quantity directly proportional to the accuracy of observation. The threshold of difference is an object of psychophysical investigation, and for this reason I believe that the theory of physical measurement ought to be based on that of psychophysical measurement.

We now turn to Professor Safford's second objection. His criticism of my use of Lagrange's formula of interpolation is twofold: first that the calculation is carried entirely too far, and second that the interpolation should have been effected by the graphic method. Before entering upon the discussion of these objections I want to say something about the general purpose of interpolation. It frequently happens that the values of a function are given for a certain number of values of the independent variable, and that one wants to know something about the values of the function for intermediate values of the argument. Every procedure which serves this purpose is called a method of interpolation. In the graphic method one plots the results on millimeter paper and connects these points by a smooth curve. Every point of the curve corresponds to a certain value of the function, which in many cases may be read off with sufficient accuracy. This is the method which Professor Safford thinks I should have employed. I have done so as a matter of fact; but I did not consider such results of sufficient interest to publish, and only published a notice in the *Archiv f. d. ges. Psychologie*, 1910, Vol. 18, p. 410, that the charts are at the command of every scientific investigator, who may be interested in them. The chief objection against graphic interpolation is that it is too arbitrary; and for this reason I relied upon numerical interpolation alone.

There is a great variety of methods of numerical interpolation; but the essential feature of all these methods is that an algebraic expression is given, which may be fitted to the course of any function. Two of the best known methods are known as Newton's method of differences and Lagrange's formula of interpolation. Both methods are essentially identical, since they both suppose that the function may be represented by an algebraic function of degree n. The greater the number of observed values is, the more reliable is the result of interpolation; and it is, therefore, desirable to have as many observed values as possible. In a scientific investigation, however, one has to take into account that the time and energy of the observer are limited and that it is better to have a few carefully made observations than a mass of not very dependable results. In planning an investigation one has to strike a happy medium, which gives as many carefully made observations as possible. Experience shows that in the study of the psychometric functions seven values of the comparison stimulus are as much as can be handled easily and effectively. I may support in this respect my own opinion by the authority of G. E. Mueller. It is obvious that one cannot make observations for every intensity of the comparison stimulus, and that one has to fall back upon interpolation, if one wants to know something about the intermediate values. I, therefore, cannot see the reason why Professor Safford should find fault with my tables, because only seven entries were original results, a fact which could not fail to be noted by anybody who read the text. Tables of interpolated values are plentiful in physics and astronomy, and no one objects to them, if they are properly pointed out as such.

I now want to call attention to a small error in Professor Safford's text and a slight inconsistency in his position. He says on p. 97 that the seven ordinates were treated as absolutely exact. This is not quite correct; the abscissæ were so treated. He, furthermore, objects to the actual set-

ting up of the equations by Lagrange's formula on account of the great number of decimals which must be retained in the coefficients, but does not raise the same objection against the interpolation without setting up the equation. The advantage of this formula is that the interpolation can be effected without setting up the equation, but the use of the formula nevertheless implies the equation. To be consistent Professor Safford should have objected to the use of Lagrange's formula in any shape, but this would have precluded the use of Newton's formula, which Professor Safford favors, because it gives the same result "and requires about one-tenth of the labor." I beg to differ on this score. I tried both methods and found that the number of figures to be written down to effect the interpolation for one intermediate value and all my seven subjects was smaller for Lagrange's formula than for Newton's method of differences.

Professor Safford's clever criticism of the equation set up for the psychometric function for Subject *I* is likely to carry the most conviction to the reader. The discrepancy between the amount of work spent in setting up the equation and the result obtained is so great that one cannot possibly help being struck by it. In this part of the work I had the good fortune to obtain the services of a professional computer, who first set up the equation with six decimal places of the coefficients only. The results of interpolation by this formula were fantastic, because the curve did not follow the actual results at all. The necessity of retaining as many decimals as were actually used later on, began to dawn upon me only after I had reasoned out that each one of these coefficients had to be multiplied by high powers of numbers around 100. I then realized the necessity of carrying out all divisions to the bitter end, and incidentally won an insight into the nature of Lagrange's formula. This formula is a merely artificial construction, the coefficients of which have not immediate physical significance at all. The formula of interpolation is a means of achieving a certain purpose and he who wills the purpose must will the means.

It is very interesting to analyze Professor Safford's criticism of my statement that the use of Lagrange's formula does not imply a definite hypothesis about the psychometric functions. The meaning of this expression, which I explained at some length elsewhere, is this. The psychometric functions give the dependence of the probabilities of the different judgments on the intensity of the comparison stimulus. We do not know anything about this dependence, but we have to make some hypothesis about it for the purpose of interpolation. This can be done in two ways: by assuming a function which fits any kind of results, or by assuming a definite law of distribution. Lagrange's formula belongs to the first class, because the degree of the function depends on the number of observations only. The form of the function is, therefore, different in different cases. A further difference between these assumptions and a definite hypothesis about the psychometric functions consists in the fact that the latter admits of an extrapolation, whereas the former as a rule, do not.

I may illustrate this distinction by the following example. Suppose that a table be given, which we know contains the values of either the sine or tangent for small angles but we do not know which. We may use Lagrange's formula for interpolating in this table, and we may represent the course of the function in this interval by this formula, but we fully realize that this hypothesis is not definitive but subject to correction and that the formula will not represent the course of the function outside the interval. If, however, we possess some further information, which leads us to believe that the tables contain the values of the function sine, we make a definitive hypothesis about the function. My monograph on statistical methods contains only a casual mention of this distinction, but the articles in the *Archiv* leave no doubt as to the meaning which I wanted to convey by these words.

Professor Safford's criticism is this: "Lagrange's formula gives the equation of a curve through n points, whose degree is not greater than n (this is not correct, it should read $n-1$; remark of the writer), and the n points determine the curve completely." There is an infinity of curves of the same type and Lagrange's formula merely has the merit of being the simplest, and it is therefore useless to spend much energy upon it. This is the standpoint of the mathematician, whose interest lies in the study of the properties of whole groups of curves. The standpoint of the practical calculator is different. He attempts to reach his goal by the shortest possible route; and I, for one, refuse to consider a method of which I know beforehand that it has no merit over another excepting that it is more complicated. Whether in a scientific investigation energy is spent uselessly or not, depends upon the importance of the results obtained; and this can be judged by the specialist alone. I do not believe that my energy was wasted in this case, for I am willing to go through all the trouble of working out my data merely for the sake of finding the result, that the maximum of the psychometric function of the equality judgments must be related to the threshold of difference.

Professor Safford sees a further objection to the use of Lagrange's formula in the fact that it excludes at once all probability curves, symmetrical as well as asymmetrical. In my opinion, this is one of the greatest advantages of direct interpolation that it fits the actual data of observation without making the assumption of a definite law of distribution. If the data follow one of these functions, the interpolation by Lagrange's formula will follow it closely enough. I may remark that I am at this point in perfect agreement with W. Wirth, who in his latest publication employs the method of direct interpolation. One of the most distressing features of the history of psychophysics is the endless discussion as to the applicability of a definite law of distribution. It was my purpose to get away from this discussion and to see how the curves would look, if the data were not adjusted according to a definite law of distribution.

Professor Safford's last objection is based on the fact that the results of direct interpolation do not agree very well with what he calls the theoretical curves. I may remark here, that I made it a point to speak of hypothetical curves and not of theoretical curves. There is, of course, only little difference between a theory and an hypothesis, because many theories should be called hypotheses; but it seems to be a pretty general rule that one does not call a doctrine an hypothesis, unless one wants to emphasize the hypothetical element in it. The supposition that the psychometric functions belong to a certain type has the character of an hypothesis in a very high degree. There is no possibility of deciding beforehand whether the data will fit such a curve; and for this reason one ought to insist on calling such an assumption an hypothesis and not a theory.[1] The fact that the actual distribution does not coincide with the hypothetical one, is very

[1] I may be allowed to state here the reason which prompted me to choose the term $\phi(\gamma)$ hypothesis instead of the customary name of Gaussian distribution. It seems to me that this term should be restricted to the distribution of errors of observations exclusively. In the work of Gauss there is no passage known, to me, which could lead us to believe that Gauss would have applied this law to empirical distributions of all descriptions. Considering the practical turn of Gauss's mind, it seems very unlikely indeed that he would have favored such an unwarranted generalization. One, therefore, ought to speak of a distribution according to the probability integral or to the $\phi(\gamma)$ function. To call this function by the name of Kramp-Laplace, as Opitz has done, makes the name a little clumsy, and is not entirely justified from the historical point of view. It is entirely inadmissible to call this function by the name of Gauss; and the chances are that Gauss himself would have been very much surprised by this honor. In the *Theoria Motus Corporum Coelestium* (*Werke*, Vol. vii, p. 238) the integral is referred to Laplace, but as a matter of fact this integral was already known to Euler. Gauss was acquainted with this fact, as may be seen from a letter dated February 10, 1810, and from a manuscript note to the *Theoria Motus*. To call the function by the name of its inventor, one has to take one's stand in a complicated historical question, for the decision of which the complete material is not yet at hand.

interesting in view of the above mentioned discussion; but it is not so much an argument against the use of direct interpolation as one against the use of some hypothetical probability curve.

After paying me some compliments as to the mathematical treatment of my problems, of which, as coming from an authoritative source, I am highly appreciative, Professor Safford remarks that my data are hardly sufficient to warrant an extensive treatment. Of course I agree that it would be eminently desirable to have a more extended material, and that it would be possible to improve upon my results; but I must insist that it is at present the most suitable material for testing the different psychophysical methods. Other statistical sciences make very extended use of mathematical methods, and their material is not always as good as that of my experiments. The work of constructing mortality tables requires efforts compared to which my work—as being done by one individual—shrinks into insignificance; but the data used do not always show the high degree of stability of those found in my experiments. This fact may readily be seen from the values of the coefficients of divergence: But few statistical investigations deal with data of a similar degree of stability. I do hope that we may soon possess an experimental material still more extended than my own; but until then my data are the best available for the specific purpose of testing the different psychophysical methods. And if one wants to make such a test one has to make the best of the material at hand.

Professor Safford has presented his criticism with admirable clearness and precision; but I am convinced that his position is untenable, and his argument is unsound. The methods of physical measurement cannot be taken over bodily and applied directly to the problems of statistics and of psychophysics. And while we are always grateful for the mathematician's interest in psychophysical discussions, yet it is a truism that every science is obliged to develop its own methods, and to grapple with its problems in its own way. That the methods and the problems of the theory of observations and of psychophysics are the same cannot be maintained; whether, indeed, any intimate relationship obtains between these two fields of scientific endeavor still remains to be determined.

BOOK REVIEWS

Dogmatism and Evolution. By THEODORE DE LAGUNA and GRACE ANDRUS DE LAGUNA. New York, The Macmillan Co., 1910. pp. iv, 259.

The term dogmatism, as employed in the title of this work, is intended "to denote the body of logical assumptions which were generally made by thinkers of all schools, before the rise of theories of social and organic evolution. Its application is therefore wider than common usage would warrant. The empiricism of Berkeley and Hume, as well as the rationalism of Descartes and Leibniz, is included in its scope" (Preface). This usage of the term is sufficiently justified by the presentation. The authors aim to make prominent the fact that empiricism and rationalism, in spite of their wide divergences, are founded upon a common basis, and that this identity of assumption is more significant for present-day philosophy than are the differences. The fundamental dogma behind both standpoints is also present in the philosophy of Kant, and in absolute idealism. The pragmatic movement, moreover, which is principally a protest against these earlier philosophies, is to some extent misdirected, since it perpetuates in a measure this self-same dogma. It suffers further from the inevitable extravagances and overstatements pertaining to doctrines that express the irritation of a reaction; and hence, while it represents an important truth, it requires re-interpretation and correction.

In presentation the first two parts of the book in particular are not only compact and closely-reasoned, but coherent and lucid. The treatment of the subject-matter can lay claim to originality, and is splendidly stimulating and suggestive. Historical rationalism and empiricism differ in that the one makes an appeal to mathematics as its ideal of knowledge, while the other relies upon introspection. In spite of this contrast, however, they both assume that experience presents us with certain unanalyzable elements, which serve as the foundation for all further knowledge. In the case of rationalism, these simple elements are universal propositions possessing intuitive certainty; in the case of empiricism, they are particulars which are capable of entering into various combinations. Hence both encounter the same difficulty, for both are committed to the view that relations are external to their terms. This is obviously true of empiricism; but it is no less true of rationalism, for the relation of inclusion cannot obtain between simple concepts, and hence rationalism must take as its starting-point, not concepts, but judgments which are indemonstrable and synthetical. Spinoza, indeed, attempts to start with a concept in which all other concepts are implicit, viz., that of substance. But the exposition runs smoothly merely because the concept is at once completely simple (or indeterminate) and infinitely determined. Rationalism fails to justify either the synthetic character of its most fundamental judgments or the passage from a system of universal truths to the 'infinite determinations' of fact in which this system finds embodiment.

This result sets the problem for Kant. The fact, however, that 'synthetic *a priori* judgments' present a problem at all is due to the assumption, which Kant shares with his predecessors, that analysis must yield final elements. "No proposition could be determined as synthetic, unless a complete definition of its terms had exhibited their ultimate disparateness" (p. 73). In other words, Kant proceeds on the assumption that pure thought supplies to experience certain universal modes of relationship to

which every experience must be subject. This assumption persists in absolute idealism. At first sight, the contrast between rationalism and absolute idealism is as great as could well be imagined. The former made relations external; the latter asserts that "the essences of things are wholly constituted by their relations" (88). The procedure of rationalism is a descent from first premises; that of absolute idealism is an ascent of which the fundamental principle of the entire scheme is the goal. Yet absolute idealism, like rationalism, is committed to the proposition that "the order and connection of thoughts and the order and connection of things are the same" (108). It necessarily depends upon an inner dialectic for the movement of its self-contained system of thought, and hence it has to choose between the claim of being able to account antecedently for all the contingent facts of history, or else to accept existing irrational facts, and thus to admit an irreconcilable contradiction in its theory of actuality.

It appears, then, that rationalism, empiricism and absolute idealism are all dogmatic, in that they all proceed upon the basis of an untested assumption with regard to the 'simple elements' or constituents of experience. On the other hand, pragmatism bases itself upon evolution and endeavors to give a functional interpretation of logical and psychological problems. With this endeavor the authors profess themselves in sympathy. Their argument, however, is intended neither as a defense of pragmatism nor as an attack upon it, but as a justification of the charge that current pragmatism is "only half-free from the grip of the traditions which it openly repudiates," and thus untrue to the deeper spirit of its own standpoint.

As a matter of presentation it is unfortunate that the authors do not connect the discussion of pragmatism more closely with the results of the preceding exposition. As they themselves admit in the preface, this omission detracts appreciably from the unity of treatment. The basis for the charge against pragmatism, it seems, is the fact that the latter has formed entangling alliances with immediatism. While pragmatism is right in its emphasis upon functionalism, its most prominent advocates have all professed their adherence to some form of immediatism. This creed is not only inessential to pragmatism as such, but is incompatible with its deeper meaning, for it introduces once more the attempt to base our thinking upon a 'simple element' or 'given.' A starting-point of this kind necessarily leads to perverted notions regarding the nature of thought. In effect it means that relations once more become external, as appears most strikingly in Professor James's contention that "the self-same piece of experience taken twice over in different contexts" is equivalent to the distinction between knower and known. The relations are treated as merely additive, *i. e.*, as exerting no influence upon the character of the experience. Essentially the same criticism applies to the pragmatic treatment of concepts. While it is true that concepts necessarily have reference to conduct, it does not follow that the nature of the concept is exhausted in any direct and 'external' relation of the given experience to a specific form of conduct. The relation of the concept to conduct is more indirect and equivocal. "From the standpoint of biological utility it is clear that the object, so far from meaning a definite type of behavior, is recognized as an object only as it is associated with important diversity of behavior in characteristically different situations" (p. 168). In other words, the concept is of necessity more inclusive than any given type of behavior. The concept cannot be identified with any conscious process, however complex, for "the group of associations which constitutes the concept may never in its entirety be present to consciousness in any single experience" (p. 170). It follows, furthermore, that "apart from this reference of thought to conduct, that is to say, in the limitless interrelations of concepts with each other, thought has as distinctive a form as any abstractly considered entity whatsoever" (p. 207).

The gist of the matter, then, as regards current pragmatism, seems to lie in the proposition that functionalism may be divorced from immediatism. On just this point, however, the position taken in the book does not seem to be altogether consistent. In the discussion of J. S. Mill's theory of objectivity (pp. 173-185), it is pointed out that Mill's fundamental mistake lies in the fact that he takes simple elements of sensation as his starting-point. These elements are held together by connections which Mill regards as 'real' but as inexplicable. The alternative proposed by the authors is that sensation is a scientific construct, that the distinction between sensations and relations is simply a matter of logical analysis. In other words, the relations fall within the experience quite as much as do the sense-elements. This is only another way of saying that objects are immediately presented—however we may see fit to interpret objectivity. Earlier in the chapter, however, the distinction between the 'given' and its relations is drawn in quite as hard and fast a way as was ever done by Mill. A passage was quoted in the preceding paragraph to the effect that the concept is never in its entirety present to consciousness in any single experience. So far as the exposition goes, there is no ground for the belief that it is ever present to any degree or in any intelligible sense whatever. On page 171 the question is raised: "How, indeed, can given conscious contents 'represent' or 'mean' or 'point to' other possible contents not given?" The answer which is suggested is that there is a tendency on the part of the associated experiences to rise to clear consciousness, and that "such inhibited tendencies to revival may affect in a distinctive manner the qualitative tone of the existing content." Such an explanation obviously fails to explain. We must either identify the concept with these nascent associations, which is incompatible with the general account given of the concept, or we are forced to recognize that the tendencies in question can, at most, effect a change in the quality or structure of what is presented or experienced, a change which may perhaps be interpreted as corresponding to the function, but which is in no sense identical with it. In other words, the relations or functions, regarded as such, necessarily fall outside the experience. That this is the intention is evidenced by the general tenor of the book, and in particular by the avowed agreement on this point with Berkeley and by the assertion that the 'real' is never experienced but always remains ideal.

The implication of the foregoing, it is evident, is that *Dogmatism and Evolution* is itself in bondage to the tradition which it accuses pragmatism of perpetuating. It is committed to the very opposition of universal and particular which it charges against the "immediate empiricism" of Professor Dewey (pp. 244, 246). In the end, the denial of immediatism is purely verbal, for the relations or meanings which are necessary to constitute things are opposed to what is 'given.' Hence we have the assertion that the 'real' is "never immediately experienced at all; it is always ideal" (p. 245). This charge, however, is significant, for it seems to indicate the source of the trouble. The immediatism attributed to Professor Dewey is essentially that of the older empiricism—the immediatism which constitutes a contrast to all forms of interpretation or mediation. It is urged, for example, that immediate experience can contain no uncertainty and doubtfulness; also that the Zöllner lines cannot be immediately experienced as convergent,—the reason in the latter case being that convergent lines are lines which when extended meet in a point, whereas the lines in question do not meet unless *conceived* as extended. In other words, if the lines are conceived as extended, the experience is held to be no longer immediate. The point of Dewey's contention, however, is that both the immediate and the mediate of ordinary philosophical usage are enveloped in a wider immediacy, and that this immediacy is meant when the assertion is made that things are what they are experienced as. Moreover, as Pro-

fessor Dewey says, this proposition is not identical with the platitude that experience is experience, but has the significance of a method of philosophical analysis. If mediation is itself immediately experienced, the proper way to find out its nature is to observe its operations as they occur, instead of applying the mediation *ab extra*, as has been done so frequently in the past.

In brief, then, it would seem that if the function and content of concepts are not immediately experienced, we are back at the standpoint of Mill, and left to derive what comfort we can from the classification of a contradiction as an 'ultimate mystery.' On this ground, moreover, we seem compelled to choose between the alternatives offered by Professor Royce, viz., a validism of Mill's type, and a world which is the embodiment of an "absolute system of ideas." At all events, we can hardly be content to say merely that the real is always ideal. On the other hand, if mediation is directly experienced, there seems to be no ground for identifying the real with something beyond experience or with any particular kind of experience. One experience is then, to all appearances, as real as another.

In conclusion the reviewer may be allowed to say that since limitations of space do not permit comment upon the many excellent discussions contained in the book, the foregoing critisicm may well seem disproportionate, in view of the many solid merits of the work. But disagreement, even though pretty fundamental, is entirely compatible with sincere respect and appreciation. Readers who remain unconvinced by the third part will nevertheless find the work one of distinct and unusual ability, a work that will abundantly repay a careful reading.

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Manual of Mental and Physical Tests. By GUY MONTROSE WHIPPLE, PH.D. Baltimore, Warwick and York, 1910. pp. ix, 534.

This manual includes a description of the apparatus and method of administration of fifty-four tests or groups of tests, a series of accounts of the chief results obtained by those who have used them, and a corresponding series of bibliographies. There is also a summary of the formulæ and tables useful in calculating central tendencies, variabilities, reliabilities and correlations from the obtained measures.

Eighty pages are given to means of measuring height, weight, head-shape and size, breathing capacity, and muscular strength, speed, precision and steadiness. The next ninety pages concern tests of sensory capacity. Under the headings 'Tests of Attention and Perception' and 'Tests of Description and Report,' we have, in the next ninety pages, carefully elaborated forms of tests in perceiving letters, words, etc., exposed by the tachistoscope, in cancelling words, letters, etc., printed amongst others, in counting dots, in reading, in adding a one place number to three given numbers in succession, in simultaneous reading and writing, and in describing and passing a detailed examination upon objects. A fourth portion of the same length covers tests of association (thinking of a word, of a word to fulfill certain requirements, and of the facts needed for simple computations), learning (to copy drawings seen in a mirror and to translate certain characters into numbers with the aid of a 'dictionary' printed on the blank), and memory (of series of digits, letters and words, and of passages). Finally, in somewhat over a hundred pages we find tests of suggestibility, imagination, invention, intellectual equipment, and developmental diagnosis. These include, often in improved forms, the size-weight illusion, Binet's other tests for suggestibility, tests of the effect of suggested warmth, Dearborn's ink-blot test, the familiar school tasks of including given words in a sentence, completing sentences and writing compositions, word-building from given letters, the Ebbinghaus 'Combination' test, Swift's interpretation of fables, Kirkpatrick's test of knowledge of the meaning of words,

Whipple's test of range of information and the De Sanctis and Binet-Simon tests of intelligence.

The tests have been chosen on the basis of thorough knowledge of the work that has been done in the field and with due appreciation of the uses to which they will be put. Although probably no student of the subject will agree with Mr. Whipple's selections in every case, all will admire their general worth and timeliness. And those who find the most to disagree with will perhaps appreciate their general worth most fully. Any one who plans to measure intellectual abilities of whatever sort, should as a first step become familiar with the tests recommended in this Manual.

The chief desiderata in means of measuring mental traits are that something of importance be measured, that the resulting quantity be objective or verifiable by any competent observer, that the precision or freedom from variable error attainable from a given expenditure of a subject's time be reasonably great, that the time and energy of the experimenter be economized, and that the results be commensurate with those hitherto obtained in measurements of the mental trait in question. There results a balance of goods in the selection or invention of a test in the case of almost every mental trait. Consequently, a practically infinite amount of ingenuity can be expended in devising tests to satisfy best these desiderata. A standard test, in the sense of an unimprovable one, probably does not now, and will not for a long time, exist in the case of any mental trait.

Professor Whipple has all these facts in mind, but, I think, in two ways does not quite maintain the most serviceable balance amongst them. In some cases he perhaps imposes too great a burden upon the experimenter in order to make too slight a gain in objectivity, precision or comparability with previous work. He is also too modest in recommending a test which happens to have been used by some one a few times, instead of devising a far better one himself. For example, counting dots (Test 27), adding a given number, say 3, in succession to three numbers and continuing with the sums thus obtained (Test 29), amenability to oral suggestions from the experimenter (Test 43), and the interpretation of fables (Test 49) have been very seldom used and could easily be very much improved.

The very difficult task of giving instructions in the administration of all these tests is well done. Often the desirable plan of printing exactly what the experimenter shall say is followed. A mass of minor information hitherto acquired at great cost of time by imitation, can thus be put into the student's hands once for all. If mental measurements are to be made by others than trained experts, such detailed instructions (possibly even still more detailed and rigorous instructions) must be accessible in print.

It would have been a great addition to the usefulness of the manual if Mr. Whipple had given approximate measures of the number of trials with each test necessary to secure a given degree of reliability. For individual diagnosis and prognosis, for measurements of change and for measurements of the relations between mental abilities, it is of very great importance to reduce the unreliability of the average or median ability found for an individual to a small per cent. Investigators commonly err by dispersing their time over too many individuals, not measuring each one precisely enough to allow straightforward inferences about anything save group averages.

Where the author does announce the number of trials to be made, I fear that he gives too few. For example, in measurements of the delicacy of sensory discrimination, he commonly requires, after a brief preliminary series, only ten judgments with the difference chosen, ten with one a little greater and ten with one a little less. It would seem that if sensory discrimination is to be measured by the per cent. of right judgments, at least fifty judgments of a given difference should be taken. If only ten are to

be taken, an arrangement to use the average error made by the subject seems preferable in many cases.

I may note also that to give only differences and permit only judgments of more . . . or less . . . relieves the experimenter from very annoying elements in the latter calculations and on the whole seems better than to allow judgments of 'equal.' The author's instructions vary on this point.

The chapter on statistical methods gives the standard formulæ with illustrations of their calculation. It is made specially useful by including the later short methods of calculating correlations. I regret that the author accepts Pearson's speculative assumption that to compare the variabilities of two series each gross variability should be divided by the corresponding central tendency. No one method o rendering the variabilities of the same group in different traits or different groups in the same trait comparable is universally valid, and certainly not the method of dividing by the central tendency. Dividing by the square root of the central tendency will be more often and more nearly right.

The summaries of work done and the bibliographies accompanying them represent a scholarly heroism all of whose sins of commission and omission will readily be pardoned by any one who has tried to do the like. The only serious fault, I think, is in quoting as measures of correlations, figures got before the effect of the variable errors of the original deviation-measures in reducing the obtained correlation from the true correlation toward zero had been discovered by Spearman. The obtained correlations of Aikins, Thorndike and Hubbell and Wissler were thus necessarily far too low. Mr. Whipple's interest in the generally neglected subject of correlations also leads him to mislead the ordinary reader by quoting resemblances of related individuals in the same trait along with the resemblances of a person's degree of ability in one trait to his ability in another. The former should be carefully explained if quoted at all in such connection.

It is to be hoped that this book and the reports that are being issued by the American Psychological Association's committee on tests will be studied and used by every investigator of human intellectual performance here and abroad. The earlier expectations from tests of human faculty on the basis of the faculty psychology, being too great, were destined to disappointment, but now that the complexity, variability and relative independence of mental functions are being understood and allowed for, we may hope for a revival of interest in inventories of individual intellects, in measuring the changes which they undergo by growth and training, and the causes of their original capacities. If Professor Whipple's work did nothing more than stimulate other investigators to measure the reliability of his tests, their susceptibility to practice effect and their value as symptoms of more general conditions, and so to amend or even replace them, it would have abundantly justified itself. It will do much more than this.

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The Phenomenology of Mind, by G. W. F. HEGEL. Edited, with an introduction and notes, by J. B. Baillie. London, Swan Sonnenschein & Co., 1910. 2 volumes. Vol. i, pp. xliv., 427; vol. ii, pp. viii., 429-823.

In Professor J. B. Baillie's recent translation we have now before us Hegel's *Phänomenologie des Geistes* in an adequate English dress. The Teuton has so far failed to make anything worth while out of this unique intellectual product from a fellow-member of his race. It is now handed over to a more distant relative, but perhaps none the less close still to the central intellectual tendencies of the Germanic races for the *rapport* necessary to its understanding, in a form to make it more readily accessible, and so to give the Anglo-Saxon a turn at its interpretation. As is, in part, implied in these two sentences, we shall probably have to approach the translation,

which as adequately represents the original as this is possible in a translation, on the assumption that it is only a means to an end, for two reasons. First, the genius of language, reflecting the habits of thought of a race, is sufficiently diverse in the English and the German to require a closer adaptation of the methods of presentation than a mere translation affords. Secondly, Hegel's presentation is, in fact, inadequate to begin with, and naturally so because he had undertaken a task to which there were no established precedents other than mere vague effort; and the great bulk of modern scientific work giving a training in accurate formulation has all come after him, so that his work needs first of all to be modernized. But the present translation will now unite the efforts of interpretation over a greatly extended area.

In a brief appreciation of such a book as the present there are perhaps essentially two things of interest to the reader. First comes the question as to the purpose of the book, and as to whether its preparation is adequate to the purpose. Second, and of equal importance would, in the present instance, probably be additional suggestions calculated to enhance the value of the book. The first has probably been sufficiently considered above; to the second we might now give a little more attention. In general, the cynical aspersion on Hegel's work of its being an intellectual travesty cannot satisfy an impartial mind upon a glance at the table of contents and a mere superficial perusal of the text of the *Phänomenologie*. Particularly, great emphasis is everywhere put on the unity of all matter of experience, and on the need of taking fully into account the presuppositions implied in our expositions in science through this unity of all matter of experience. This receives practically no attention in present-day science, while its recognition would bring about a far reaching revolution in science. To get into Hegel's analysis in the present work, a very close study of his Introduction, with all the implications, is advisable for the necessary point of view. This point of view is not that experience as a whole must be explained in terms of some single definite conception, as idealism, monism, materialism, psychophysical parallelism, and so on; but that these very conceptions themselves are all elements in the form of stages in this experience, while the single fact of experience as a distinct conception is nothing for us. Perhaps due largely to its subject-matter, the English rendering of the Introduction is particularly good, so that not even an original reformulation in the English could improve it much. One suggestion might here be made, which has reference more especially to the genius of the English language and the Anglo-Saxon habit of mind. If in place of the abstract noun representing completed action the present participle of continuing action were more generally used, or if the reader will more generally supply the sense of a continuing for that of a completed action, the representation of the original would be more exact; as for instance near the top of page 78, by putting for *die Darstellung des erscheinenden Wissens*, "the presentation of a developing knowing" instead of "the exposition of knowledge as a phenomenon." This will make awkward English, but it more adequately represents the continuous flux of conceptions as necessary to understand Hegel, contrasting with the fixedness or permanent demarcation of conceptions in the English.

It might be to the point to consider this flux of conceptions in Hegel's method a little farther; and the real nature of this flux is best shown by its formal recognition in the antinomy. In the antinomy, we have a contrast of two mutually exclusive or incompatible appearances of the same matter of experience of such a nature that one of them complete and distinct, but only one, is necessarily present while the other is impossible for the time being. Upon close examination and analysis of the appearance present, however, it literally dissolves, as if by magic, under our very eyes; and its contrasting opposite appears and immediately takes its place. Moreover, only at the completion of the actual perversion are the full meaning and all

implications of each of the two forms of appearance of the experience wholly understood. Classic among such antinomies are those regarding the nature of motion in our environment developed among the ancient Greeks; regarding the innateness of ideas, developed by ways of reaction between Descartes and Locke; regarding the nature of the reality of the matter of our experience, developed by Berkeley and Hume; and the cosmological antinomies of Kant. Besides these particular instances, the antinomy can, however, be found permeating our experience everywhere. Through it is revealed an actual single inner movement and unity, in the universe as it surrounds us, totally different from the cosmic motion of the Copernican system and not recognized in present-day science but which alone will account for such perplexing phenomena as the action of force at a distance. The fixedness of conception, then, as most characteristic of the Anglo-Saxon mind, here invariably leads to confusion and a sense of loss of reality with the passing of the forms present, because the continued presence simply of the fixed forms is taken to be the reality, while philosophical analysis invariably shows that the forms cannot be so retained. The Introduction, from page 82 to 89 of the translation, read in the light of this suggestion, may prove to be of more value, reference being had not only to the fact of flux, but also to the mode of flux, of conceptions.

Futhermore, Hegel in his analyses exhibits a kind of pedagogical ungainliness, which borne in mind will explain and clear up no small part of his obscurity. So the following summary may serve to guide and elucidate. The "Meaning" in the section heading on page 90 of the translation should be understood in the sense of "Supposing;" for this beginning section, subordinated under the general head of Consciousness, is a discussion of the *sinnliche Gewissheit eines gemeinten Diesen*, the sensations or sensuous awareness of a supposed this as apart by itself, over against the knowing, which is the beginning of all conscious knowledge. The next sub-section under the head of Perception (page 104) examines the development and nature of the unities in things, under another aspect also called universals, as essentially forming the basis of our conceptions of the nature of things. The third sub-section under the head of Understanding (page 124) discusses the formation of a continuous scheme of things constituting our conception of the nature of things and consisting of two fundamentally distinct parts or elements, the physical occurrence or fact of experience, and the apperceptive content or metaphysical part in an Aristotelian sense. Following these three sub-sections under the general heading of Self-consciousness (page 163) is shown the mode of appearance of a persisting self possible only through a succession of antinomies or course of antinomic dialectic. The preceding sub-sections show the essential elements in ordinary knowing in their more or less independently distinct character; in this section we have these elements, mutually dependent, forming an indissoluble system. The rest of the work, beginning with a section under the head of Reason (page 220), then shows how out of the movement of successive reactions between the antinomic moments, in a single movement, of a persisting self knowing and an other than the self known, develops the comprehensive structure of science, the conventionalized conscious content of our experience. A closing section under the head of Absolute Knowledge (page 800) then considers various characteristic incidents of the independent absolute form attained in our experience by the foregoing mode of analysis.

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De l'origine et de la nature mnemoniques des tendances affectives. Par E. RIGNANO. Estratto da "Scienticia": Rivista di Scienza. Vol. 9, Anno 5 (1911). N. XVII. 1. Traduit par le Prof. J. Dubois. 35 p.

In this article the author outlines a genetic theory of the affective states. The term affective is restricted to the special category of organic tendencies

which manifest themselves subjectively in man as desires, appetites or needs, and which objectively, in both man and animals, are translated into non-mechanized movements. Admitting this definition, the author reduces the entire series of the principal affective tendencies to the single fundamental tendency of the organism toward its own physiological state of equilibrium. This tendency may be observed in all unicellular organisms, *e. g.*, hunger which is the most fundamental affective state is reducible to a tendency to maintain or re-establish in the nutritive internal *milieu* the qualitative and quantitative conditions which permit a stationary metabolic state. This tendency toward a state of metabolic equilibrium has become, in the course of phyletic evolution, a tendency to accomplish all the acts necessary to procure food. The hydra and sea anemone, for example, react positively to food only if the metabolism is in such a state as to require more material. The localization of hunger in the higher animals is a secondary development, and merely one of the multiple aspects of the part functioning vicariously for the whole which characterizes all the physiological mnemonic processes. It is the same with thirst, which though localized in the glands of the throat, is a need of the entire organism. Similarly, the need of elimination of substances produced by general metabolism, which the organism is not able to utilize, whether in the simplest infusoria or the most complicated vertebrate, follows the same general rule. In this category of affective eliminative tendencies, the author places the sex instinct. To this fundamental property that every organism possesses, *i. e.*, the tendency to conserve the equilibrium of its own normal physiological state or to re-establish it if it has been disturbed, must be added another which in its turn becomes the source of new affectivities. When the original state cannot be re-established then the organism tends to pass into a new static condition adapted to the new external or internal *milieu*. In this way, the whole series of the phenomena of adaptation is produced. The experiments of Dallinger and others on the acclimation of lower organisms have shown conclusively that this secondary state once established tends to perpetuate itself. This tendency is of a purely mnemonic nature and implies for the different elementary physiological states, forming altogether the general physiological state, the faculty of leaving behind a specific accumulation or mnemonic residue susceptible to revival or recall. The extension of this faculty of specific accumulation to all physiological phenomena in general is in harmony with the hypothesis which posits nervous energy as the basis of all vital phenomena.

With the extension of this mnemonic faculty to all the elementary physiological processes, we arrive at a somatic or visceral theory of fundamental affective tendencies. Naturally in organisms endowed with a nervous system there would gradually be developed along with the affective tendencies whose origin is purely somatic, the tendency, sometimes co-operative and sometimes vicarious, "represented by the corresponding mnemonic accumulations, deposited in that special zone of the nervous system which is directly connected with respective points of the body." In man, this zone would be the *Körperfühlssphäre* of Flechsig, to which is added in certain cases the frontal zone. These mnemonic cerebral accumulations once established under direct somatic action are able, even after communication with the body has been severed, to represent the original affective tendency in which they originated, *e. g.*, Sherrington's "spinal" dog showed the same repugnance to dog flesh in precisely the same way as a normal dog. Finally affective tendencies owe their subjectivity to their mnemonic, physiological origin, from the fact that the organism finds itself endowed with specific affective tendencies in accordance with the particular environmental history of the species or individual. In support of the foregoing hypothesis thus briefly sketched, the author cites various examples from the higher animals and man, *e. g.*, he finds the origin of maternal love in

the principle of elimination, the need of being nursed. Homesickness is due to the disturbance of fixed paths of habituation. As a further confirmation of the hypothesis of the mnemonic nature of the affective tendencies Ribot's principle of *transfert* is utilized.

In accordance with this principle, in itself of mnemonic origin, all affectivities not directly traceable to a mnemonic source are derived from those which are thus referable, and are therefore of indirect mnemonic origin, *e. g.*, secondary sex affectivities, cruelty as an end in itself, derived from the original tendency of tearing prey to satisfy hunger, the desire of victory for itself, originally self defence, the desire of amassing wealth, which is a transfer from the original simple impulse to satify hunger plus the intellectual element of foreseeing its recurrence.

Emotions according to this theory "are only sudden and intensive modes of putting in action those accumulated energies, which constitute precisely the affective tendencies." Emotions and affective tendencies are distinguished from each other by the fact that the same affective tendency may, under different circumstances, give rise to very diverse emotions; to emotions of different intensities or even, in some cases, to no emotion at all in the proper sense of the word, *e. g.*, the affective tendency of a dog for his piece of meat may be translated, according to circumstances, into flight, anger, or merely a search for a quiet place in which to enjoy it. As all affective tendencies result in movement, external or internal, the theory is here in accord with that of Ribot and the Lange-James theory.

The will is only an affective tendency inhibiting or impelling to action like every other affective tendency. As to pleasure and pain, the theory is in accord with that which interprets pleasure as the subjective accompaniment of unimpeded activity and pain as due to its inhibition.

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A Text Book of Psychology. E. B. TITCHENER. Macmillan Company, New York, 1910. pp. xx + 565.

Professor Titchener in the present volume has given us more than a text-book of psychology. The book comes fairly close to being a brief, systematic psychology — an earnest, certainly, of what the author will give us when his more complete study is ready. At the present time, in English, we have at our disposal many elementary texts on psychology, and many elementary laboratory manuals; but we are poverty-stricken for advanced works in general, systematic psychology (based step by step on experiment), and for advanced experimental manuals on the various sense-fields,—attention, association, etc. Titchener's book, while supposed to be for elementary students, is far from being an easy text. Indeed, the author's own way of thinking has become so much more complex since the writing of the *Outline* that I doubt if he himself clearly realizes just how much of his more recondite reflections have become incorporated in the book. If I were seeking a quarrel with the Text-book I should find the grounds for it on the score of too great complexity. It is a little heavy for the average junior or senior. But psychological classes differ greatly in the different institutions. In some, psychology is required; in others, elective. In some the "quarter' system is in vogue, and only one quarter is allotted to psychology; in others, psychology runs the year through. It is doubtful whether Titchener's book can be adapted to meet the requirements of a short course. In institutions where the elective system is in operation, and where a full year can be given to psychology, I know of no text better to use than the one under discussion. In view of the fact that the author introduces experiments everywhere and that he discusses methods and results the book lends itself easily both to systematic and to experimental presentation. Any student going carefully over the work with a competent instructor will come out at the end of the year with an increased respect for psychology and with the

ability to think along psychological lines and to read and follow the future progress of psychology even if he carries his training no further.

In a book which is so full of factual material, we cannot hope in a review to discuss chapter and verse in any adequate way. Certain interesting points of view developed by the author, alone can be discussed. In the first place, Titchener's series of chapters on sensation is excellent—by far the best treatment we have. For, in addition to the full treatment of the ordinary laws and principles involved in sensation, we have the more recondite phenomena touched upon. Much additional material over that treated in the *Outline* appears. For example, we have a fuller treatment of color-theories; of the vestibular and ampullar senses; of the sensitivity of the abdominal tissue; of sensations arising from the digestive and urinary systems; and from the circulatory and respiratory system.

In discussing the attributes of sensation in general, the author tells us that there are four distinct attributes; quality, intensity, clearness and duration. The reviewer is puzzled by the attribute *clearness*. We all admit clearness as an attribute of complex conscious experience, but not as a fundamental aspect of the sensation-process—not in the sense in which duration and intensity are attributes. He says, "Clearness, again, is the attribute which gives the sensation its particular place in consciousness; the clearer sensation is dominant, independent, outstanding; the less clear sensation is subordinate, undistinguished in the background of consciousness." This is certainly to be admitted, but surely what Titchener is writing of here is an attention-state, in which a given "sensation" is focal, while others appear in what James calls the "fringe." In other words, clearness is one of the descriptive words which we apply to perceptual, ideational and other complex mental states. With this given as an attribute of sensation one would expect to find it taken account of somewhere along with the other attributes of sensation. But in his chapters on the special senses he speaks only of the usual attributes of each group, introducing certain changes in terminology, to be sure, as for example, he speaks now of the qualitative attributes of a color as being hue, tint and chroma.[1] And further, in audition, he speaks of size and diffusion as an attribute of tone. It would seem in places that he means to use this attribute of clearness in the same sense as we should use *clearness* in describing a perceptual state; but this would carry with it the inference, it seems to me, that sensation is something more than an abstraction—something that can actually present itself. Futhermore, in order to realize the conditions for the appearance of clearness, we should have to have at least two such "sensation processes" attempting to run their courses simultaneoulsy. But this is certainly the process which we know as perception. The confusion, if I understand Titchener's statements, is similar to that found in James where sensation is at times discussed as an abstraction and at others as a process corelative with perception.

It is interesting to note that he treats of the sense-image under the general chapter heading of synæsthesia; since the image is the normal process, and synæsthesia the anomalous one, we should suppose that the traditional order of treatment were best. One would hardly begin a chapter on color vision for elementary students with a discussion of red-green blindness. His early discussion of imagery is rather disappointing. Only two pages are given over to it. One finds there few statements concerning the experimental mode of investigating the image, and very little of individual differences. This lack of emphasis of the image in an early place would seem to be a real limitation in the use of the book as a text. The average undergraduate rarely wakes up to real introspective interest in psychological problems until he has learned that he has imagery and can stand

[1] His introduction of the words chroma and tint are of doubtful value, since the word saturation, now in common use, seems adequate.

off and look at it, as it were, in the absence of a perceptual world. A brief study of the image awakens him far more rapidly than does a much longer drill on sensation-processes. Later on in the book, however, the author completes the treatment of imagery under the headings, association, memory and imagination. Here the treatment is full and adequate.

Following the chapter on synæsthesia is one on the intensity of sensation, which includes a discussion of mental measurement, liminal and terminal stimuli, just noticeable differences, and Weber's law. The chapter is concise, but clear, and since these topics are valuable to the student, such a chapter is a real contribution on the pedagogical side.

Then follows the chapter on affection. He stands by his position stated in the *Outline*. "The writer holds that there is an elementary affective process; a feeling element. . . ." "He holds further that there are only two kinds or qualities of affection, pleasantness and unpleasantness." Although the reviewer thinks he finds himself in another 'camp,' it gives him a sense of security to find a psychologist of Titchener's eminence who admits his position so frankly on such a vexed question as that of affection. On page 228, in a discussion of the relation between sensation and affection, he again enumerates the attributes of sensation—*quality, intensity, clearness, duration*. Those of affection on the other hand, are *quality, intensity, duration*. On page 231, he uses clearness as the distinguishing criterion between sensation and affection. "Pleasantness and unpleasantness may be intensive and lasting, but they are never clear." This is due, in short, the author says, to our inability to *attend* to an affection. "The lack of the attribute of clearness is sufficient in itself to differentiate affection from sensation. . . . . " Again, this attribute of clearness attaching to sensation and not to affection, and further the fact that we cannot attend to affection, make the author reject the view that affection is really a complex or fusion of the accompanying organic sensations. While there is no time to argue the question, I cannot see that Titchener makes his point against this latter view. If we should grant his premises, namely, that affection lacks clearness, and that it cannot be attended to, we should be forced to admit his point. But these are just the questions at issue.

He discusses two methods of investigating affection: that of "paired comparison," and the method of "expression." He devotes about six pages to the method of expression, but is in agreement with the majority of psychologists in denying any great usefulness to it. It seems like a useless luxury in a text-book to treat so at length of a method which has absolutely nothing to recommend it. The tridimensional theory of feeling is well and critically diccussed.

Space does not permit of even a brief review of further chapters; attention, perception, association, memory and imagination, action, emotion, and thought. The chapters on attention and on thought are striking and are both readable and teachable.

In the chapter on action it is with a shock that one again meets with his earlier expressed view that the first movements of organisms were conscious movements (agreeing thus with Wundt, Ward and Cope). According to this view, voluntary action degenerates into ideo-motor or sensory-motor action, and then into the reflex. But in animal life we find two kinds of responses, in every organism, beginning with the protozoa (as shown by the recent work of Gibbs and Smith, of Bentley and others): the one type being fixed and definite; the other diffuse, leading itself to habit-formation. Certainly I should agree with Titchener that consciousness is as old as life, but I should certainly connect consciousness with the diffuse type of response. I should say further that *the very first organism started with both types of response*. Surely nothing is gained, and confusion is introduced by the conception of Titchener that all movement was first a voluntary acquisition, and that only later do we begin to find fixity

in the responses of organisms. There is not a scintilla of evidence that *fixed* and automatic reactions do not appear with the first appearance of organisms. And there is abundant reason to say that each new species as it appears, *e. g.*, by mutation (see the work of Tower *et al.*), gives evidence of a reflex repertoire and of a plastic repertoire. Titchener argues that the reason we do not see this complete plasticity (which would be called for on his theory) in the unicellular form to-day, is that the protozoa have travelled all the way from plasticity to fixity.

Looking at the book as a whole, it seems to the reviewer that in many places Titchener has adhered too rigidly to the introspective method. Surely in his treatment of *meaning* he could have leaned advantageously a little way toward the functional side, without giving up the guiding principles of the book. Nevertheless in this day when, if I can read the signs aright, the pendulum is swinging another way—toward a study of life-situations as a whole, and the adequacy, permanency and different types of adjustment which such situations call forth—Titchener gives us an enviable example of a man unafraid of his own views of the problems of psychology, and of his own methods of solving them. JOHN B. WATSON

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L'année psychologique, publ. par A. BINET, avec la collaboration de LARGUIER DES BANCELS et Dr. TH. SIMON, etc. Seizième année. Paris, Masson et Cie. 1910, pp. IX, 500.

The introduction reviews the progress of psychology in 1909, treating especially the work on thought and on pathological states, and the work in experimental pedagogy and judicial psychology. The first original contribution, by A. Binet, is entitled "The physical signs of intelligence in children." Greater or less degrees of correlation are found between intelligence and size of head, the so-called signs of degeneracy (abnormally shaped head, ears, etc.), facial expression, and hands. The habit of biting the finger-nails is found to be without significance in this respect. The correlations found hold in general only for the group, not always for the individual. The physical signs are useful for confirming, rather than for making, estimates of intellectual level. Next in order is an examination of the art of Rembrandt, by A. and A. Binet. The authors attempt to show how, by avoiding extremes of contrast and by accentuating unity of lighting, Rembrandt has succeeded in giving those impressions of distance, of unity, and of light which characterize his work. "Tachistoscopic Researches," by B. Bourdon, is an investigation of the times of choice-reactions made by observers to whom colors, rectangles of different lengths and figures, have been tachistoscopically exposed. The writer measures the time of reactions involving judgments of resemblance, identification, localization, comparison, or combinations of these processes. The eight following papers, by A. Binet and Th. Simon, are concerned with defining the various mental derangements. Up to the present, the authors believe, definitions have been too inclusive and general, have failed to show the *essential* characteristic which marks off the disorder, and have been couched in terms only partly psychological. They themselves classify the derangements as (1) hysteria, (2) derangement with knowledge (fears, impulsions, etc.), (3) manic-depressive insanity, (4) systematized insanity (paranoia), (5) the dementias (general paresis, senile dementia, dementia præcox), and (6) subnormality. They consider the history of the conceptions of the various disorders, the theories propounded and the attempts at definition. They also review the characteristic mental states, symptoms, and attitudes of patients, both as reported by others, and as shown by the new data here published. They compare the special derangement under consideration with the other types of derangement, and finally arrive at a conclusion as to its essential character. Of hysteria, they find character-

istic, separation; of derangement with knowledge, conflict; of manic-depressive insanity, domination; of paranoia, deviation; of the dementias, disorganization; of subnormality, arrest of development. The psychological significance of these terms is discussed and explained at length. and an attempt is made to bring them all into relation. "Judicial Diagnosis by the Association-method," by A. Binet, argues against unlimited confidence in the method for application in practice. The writer reviews the experiments of Henke and Eddy and of Yerkes and Berry, pointing out chances for error, and showing on both theoretical and practical grounds that the method, as used in the laboratory or classroom, is not suited to the conditions of the criminal court. The psychological literature of 1909 is reviewed by Beaunis, Binet, Bovet, Larguier des Bancels, Maigre, and Stern, under the headings of physiological psychology, sensations and movements, perceptions and illusions, associations, attention, memory and images, language, feelings, æsthetics, thought, suggestion, individual psychology, child psychology and pedagogy, animal psychology, judicial psychology, pathology, dreams, treatises and methods, and philosophical questions.

W. S. FOSTER

Il sentimento giuridico. GIORGIO DEL VECCHIO. Seconda Edizione. Roma: Fratelli Bocca, 1908. pp. 26.

Professor del Vecchio, of the University of Sassari, who has previously published several articles on kindred topics,—*L'etica evoluzionista* (1903), *Diritto e personalità umana nella storia del pensiero* (1904),—discusses in the present monograph the "feeling (or sense) of justice" in man, its origin and development. From the time of Aristotle down this "sense of justice" has been attributed to man, but the philosophers have disputed much as to its primary or derived character (these arguments the author briefly summarizes). According to Professor del Vecchio, "the origin and nature of the sense of justice is essentially a problem of the metaphysical order" (p. 12). This, however, does not prejudice in any way the analysis of the psychic *datum* and its proper functions. The "sense of justice" is thus "primary and normal *datum* of the ethical conscience, an element or an aspect of it; and its nature is affective and, at the same time, ideological." A fundamental and distinctive characteristic of the "sense of justice" is its independence of all exterior sanction,—that is just, which is right independent of all positive historical sanction. Thus justice and law differ. No prescription of law can destroy this original faculty of conscience to oppose itself, as supreme principle, to the authority of constituted law (p. 23), this, Hobbes to the contrary notwithstanding. The philosophy of law is rooted in the "juridical vocation of conscience." The "sense of justice" is "the anthropological exigence of law, its primary indication, and the psychic expression of its human necessity."

ALEXANDER F. CHAMBERLAIN

Sulla Craniologia degli Herero. DOTT. SERGIO SERGI. Roma, 1908. pp. 10. (Estr. dal *Boll. d. R. Accad. Med. di Roma*, Anno XXXIV, Fasc. I).

Contributo allo Studio del Lobo frontale et parietale nelle Razze umane. Osservazioni sul Cervello degli Herero. Pel DOTT. SERGIO SERGI. Roma: Fratelli Pallotta, 1908. p. 107, 1 pl.

In the first of these studies Professor S. Sergi gives the results of his examination (description, measurements) of 6 male crania of the Herero (a Bantu people of Damara Land, German Southwest Africa), now in the collection of the Anatomical Institute, Berlin. The skull-capacities range from 1,315 to 1,590 ccm. (4 are 1,500 or over); the cephalic indices from 67.5 to 72.9 (4 below 71). The Herero have a skull-capacity approaching that of the Kaffirs of the S. E. coast,—it is a curious fact that the Bantu peoples of the S. W. and S. E. coasts have a cranial capacity greater than that of those of Central Africa and the region of the upper Congo. The cephalic index of the Herero ranks them among the more dolichocephalic

Bantu. Previous to this paper, but two Herero skulls have been studied (one by Fritsch in 1872, the other by Virchow in 1895).

In his monograph on the brain of the Herero Professor Sergi treats in detail of 14 young adult brains (male 11, female 3) in the Anatomical Institute of the University of Berlin, with special reference to the frontal and parietal lobes. A few of these brains had been previously investigated in a general way by Waldeyer in 1906. Anatomical description and measurements are exhaustive: fissure of Sylvius, fissure of Rolando and the relative development of the frontal lobe, sulci of lateral surface of the frontal lobe, sulci of the orbital surface, fronto-parietal median sulci, sulci of the cranial surface of the parietal lobe, etc. Comparisons are made with similar data for other races, and the 8 figures in the accompanying plate demonstrate well the anatomical facts, by reference to the text-descriptions. The weights of the fresh brains range from 1,146 to 1,470 gr. (the 3 female are all below 1,200; 6 of the male below 1,300 and 2 above 1,400),—the Herero are said to average 1,800 mm. in stature, with head relatively small as compared with the body. Intellectually the Herero are inferior to the Hottentots, whose skeleton and musculature are of a finer structure (their average height is 1,700 mm.). Both Herero and Hottentots belong to the Bantu division of the Negro Race. Some of the facts brought out show how dangerous it is to generalize, *e. g.*, for "all Negroes," as Parker does with respect to the direction of the Silvan fissure. In the method of termination of this fissure the Herero show 17 simple and 11 bifurcate, a proportion close to that of the Polish brains studied by Weinberg (Javanese, Swedish, Lett and Esthonian brains show a large majority the other way). The development of the upper and lower frontal lobe is more variable in males; and in both males and females more variable on the right than on the left. The absolute development of all the frontal lobe is greater in males than in females. The fissure of Rolando is more irregular on the right in male brains, on the left in female. In male brains left rami, in female right rami predominate. As has been shown for the *Hylobates*, the facts indicate, according to Professor Sergi, the existence in the human frontal lobes of two distinct zones, an upper and a lower, which follow different laws of development. Of these "the upper left has in female brains reached its proportional development with respect to the other parts of the brain, while in male brains has still a considerable evolution to undergo" (p. 40). In the greater frequency of the separation of the inferior frontal sulcus from the precentral and the less frequency of a close anastomosis between them, the Herero brains differ from those of all other races so far examined. In the Herero the upper and lower frontal sulcus shows more divisions than in the European. With respect to the varieties of disposition of the retrocentral sulcus the Herero brains "reveal neither a condition of ontogenetric arrest, nor a phylogenetic record" (p. 83). While not venturing to draw any dogmatic general conclusion from the facts recorded, the author feels authorized to make this statement (p. 103): "The more rational analytical method for the determination of the variability of the cerebral sulci is still in its infancy waiting for the aid of microscopic research; and at present it can be asserted that we do not know a single morphological characteristic of the cerebral sulcature, which belongs exclusively to a given human race. But the frequency of determinate variations indicates sometimes the tendency toward oscillations and divergences, which, with certain limits, seem to be proper to a given human group; but more noteworthy still is the tendency toward the persistence of certain morphological characteristics of the cerebral sulcature in relation to sex independently of all ethnic differences."

A complete analytic study of all the Herero brains here considered will be found in Professor Sergi's more extensive monograph *Cerebra Hererica* to appear in "Ergebnisse einer zoölogischen Forschungsreise in Südafrika mit Unterstützung der Kgl. Preuss. Akademie der Wissenschaften zu Berlin von Dr. Leonhard Schultze." ALEXANDER F. CHAMBERLAIN

BOOK NOTES

Das Bewusstsein, von JOHANNES REHMKE. Heidelberg, Carl Winter, 1910. 250 p.

Philosophes et penseurs. Buchez (1796-1865), par G. CASTELLA. Paris, Bloud, 1911. 64 p.

Institut de Sociologie, Bulletin Mensuel. No. 1, Janvier, 1910. Instituts Solvay, Parc Léopold, Bruxelles.

Philosophes et penseurs. Léonard de Vinci, par LE BARRON CARRA DE VAUX. Paris, Bloud, 1910. 62 p.

Kant and Spencer. A study of the fallacies of agnosticism, by PAUL CARUS. 2d. ed. Chicago, Open Court Pub. Co., 1904. 107 p.

Kant's Prolegomena to any future metaphysics, edited in English by PAUL CARUS. Chicago, Open Court Pub. Co., 1902. 301 p.

The fundamentals. A testimony to the truth. Vol. 4. Compliments of two Christian laymen. Chicago, Testimony Publishing Co., n. d. 128 p.

The concept of method, by GERHARD R. LOMER. Published by Teachers College, Columbia University, New York City, 1910. 99 p. (Contributions to Education, No. 34.)

Subconscious Phenomena, by HUGO MÜNSTERBERG, THEODULE RIBOT, PIERRE JANET, JOSEPH JASTROW, BERNARD HART and MORTON PRINCE. Richard G. Badger, Boston. 1910. 141 p.

On the genesis and development of conscious attitudes (Bewusstseinslagen), by WILLIAM FREDERICK BOOK. Reprinted from the Psychological Review, November, 1910. Vol. XVII, pp. 381-389.

Transactions of the Congress of American Physicians and Surgeons. Eighth Triennial Session, held at Washington, D. C., May 3rd and 4th, 1910. New Haven, Conn., published by the Congress, 1910. 456 p.

The influence of complexity and dissimilarity on memory, by HARVEY ANDREW PETERSON, PH. D. Dissertation, University of Chicago; Monograph Supplement, No. 49, of the Psychological Review. n.d. 87 p.

Im Kampf um die Tierseele, von J. VON UEXKÜLL. Separat-Abdruck aus Ergebnisse der Physiologie, 11. abt., hrsg. von L. Asher in Bern und K. Spiro in Strassburg I. E. Wiesbaden, Bergmann, 1902. 24 p.

The first principles of heredity, by S. HERBERT. London, Adam & Charles Black, 1910. 199. p.

This compend on heredity has sections on the germ cell, theories, inheritance of acquired characters, of diseases, Mendelism and biometrics, with its conclusions.

Text-book of nervous diseases for physicians and students, by H. OPPENHEIM. Authorized translation by Alexander Bruce. Edinburgh, Otto Schulze & Co., 1911. 2 v.

This is the fifth, enlarged and improved edition with 432 illustrations in the text and 8 plates.

Report of the committee of the American Psychological Association on the standardising of procedure in experimental tests. Committee: CHARLES HUBBARD JUDD, WALTER B. PILLSBURY, CARL E. SEASHORE, ROBERT S. WOODWORTH, JAMES R. ANGELL, Chairman. Published by the Association. The Psychological Monographs, Dec., 1910. Vol. 13, No. 1. 108 p.

The evolution of mind, by JOSEPH MCCABE. London, Adam & Charles Black, 1910. 287 p.

The writer discusses the lowest and earliest forms of life, appearance of the brain, development of the fish, invasion of the land, insects and intelligence in them, mind in the bird, growth of the mammal brain, law of heredity, and advance in man.

Die innere Werkstatt des Musikers, von MAX GRAF. Stuttgart, Ferdinand Enke, 1910. 270 p.

This work treats of the unconscious, how it has affected romantic and classical productions in the field of art, the creative mode, artistic conception, outer impulse and inspiration, critical work, the sketch, technique, the classical and the great style.

Heredity in the light of recent research, by L. DONCASTER. Cambridge, University Press, 1910. 140 p.

The writer considers variation, its causes, a statistical study of heredity, Mendelian heredity, disputed questions, heredity in man, historical summary of theories, the material basis of inheritance. The work is all it claims to be, a simple introduction to the subject.

Vom Selbstgefühl, von ELSE VOIGTLANDER. Leipzig, R. Voigtlander, 1910. 119 p.

After a general characterization of self-feeling, the author gives its types, vital, self-conscious, etc. Then she discusses mirror-consciousness and its various forms. The writer's point of view is original and naïve. She goes to nature rather than to books for data.

Das vorgeschichtliche Europa, Kulturen und Völker, von HANS HAHNE. Monographien zur Weltgeschichte, herausgegeben von Ed. Heyck. Bielefeld, Velhagen und Klasing. 1910. 130 p.

This is a very interesting and comprehensive compend, with illustrations on nearly every page, the whole designed to give the beginner a general survey of the results, up to date, of the investigations into prehistoric times in Europe.

A study of association in insanity, by GRACE HELEN KENT and A J. ROSANOFF. Reprinted from The American Journal of Insanity, 1910. Vol. LXVII, Nos. 1 and 2, 142 p.

This work, on the background of association in normal subjects, passes to that of a number of insane people, giving stimulus and reaction-words, and making careful generalizations from a large number of cases, and finally analyzing out certain symptoms.

Hereditary characters and their modes of transmission, by CHARLES EDWARD WALKER. London, Edward Arnold, 1910. 239 p.

This is an interesting text-book beginning with the cell and passing to instinct, theories of evolution, mutation hypothesis, continuity between species, protective coloring, law of frequency, immunity to disease, Galton's theories, environment, trypanosomes, ants and bees, Mendel's experiments, breeding, sex determinants, etc.

Zeitschrift für Psychotherapie und medizinische Psychologie, von ALBERT MOLL. Ferdinand Enke, Stuttgart, 1909. 1 Band, 384 p.

In this first volume we have a very imposing array of articles by eminent experts making original contributions to the subject. The references, too,

and the record of sittings, with a miscellaneous section, make a good and very interesting and attractive collection of view-points in a subject which at present is rather unusually lacking in harmony.

The age of mammals in Europe, Asia and North America, by HENRY FAIRFIELD OSBORN. N. Y., Macmillan, 1910. 635 p.

The writer has here brought together a very valuable report of the state of the topic under discussion, together with his own investigations which have been comprehensive. He divides his work by geological periods, eocene, oligocene, miocene, pliocene, and pleistocene, discussing under each the characteristics forms found in different countries. He does not enter the field of man.

The book of the animal kingdom; Mammals. By W. PERCIVAL WESTELL. London, Dent, 1910. 379 p.

Perhaps the best thing about this book is its many and excellent illustrations from life, too often, alas! life in captivity, of the many mammals described. A number of the best colored cuts are reproductions of extinct forms of life. Special attention is given, too, to the rarer and remoter forms. The work is rather popular, and approximates what a boy's animal book ought to be.

Individualism, by WARNER FITE. New York, Longmans, Green & Co., 1911. 310 p.

This book is four lectures on the significance of consciousness for social relations, given in 1909 at the Summer School in Chicago, but here very greatly developed. The author's general position is insistence upon individualism, *versus* the present emphasis laid by men like Dewey and Royce upon social relations. Indeed, the book is in part a friendly criticism of the views of these authors.

L'état mental des hystériques, par PIERRE JANET. Paris, Félix Alcan, 1911. Deuxième edition. 708 p. (Travaux du laboratoire de Psychologie de la Clinique à la Salpêtrière—Cinquième serie.)

This is simply a reprint of the first edition of the first volume of the author's work which was published in 1893 and 1894 and it is here reprinted almost exactly, without change, because the author found that he must choose between this method and that of radically reconstructing his work, and because much of this volume is devoted to plain descriptions of cases valid under any theory.

Moto-sensory development: Observations on the first three years of a child. By GEORGE V. N. DEARBORN. Baltimore, Warwick & York, Inc., 1910. 215 p. (Educational Psychology Monographs.)

This is a study of the author's own child from birth up to the 152nd week. The frontispiece is the baby itself, and there are notes on observation, which constitute the bulk of the book; certain inductive considerations; a chronological epitome of observed development which presents salient facts in a condensed way; and lastly, the various first appearances are alphabetically arranged.

Mentally deficient children, their treatment and training, by G. E. SHUTTLEWORTH and W. A. POTTS. 3d ed. Philadelphia, Blakiston 1910. 236p.

This is a new edition of an almost standard work and contains some additional material. After an historical retrospect there follows a characterization of feeble-minded, degenerate and epileptic children; then comes a description of the instruction they require, pathological classification, etiology, diagnosis and prognosis, mental examination of children requiring special instruction, treatment, intellectual, industrial and moral training, recreation, with results and conclusions. An appendix lists institutions in England and America, gives speaking exercises, and a bibliography is appended.

Modern theories of criminality, by C. BERNALDO DE QUIRÓS. Tr. from the Spanish by Alfonso de Salvio. Boston, Little, Brown, and Co., 1911. 249 p.

This is an admirable survey and begins with origins, laying special stress upon Lombroso, Ferri and Garafalo. Then the writer discusses theories of anthropology, degeneration, pathology, sociology, parasitism, criminal tendencies. The book was written to furnish Spanish scholars and jurists with a general survey of what is being done in this field. This commission which has been given the author has been admirably executed by him.

Questioned documents. A study of questioned documents with an outline of methods by which the facts may be discovered and shown. By ALBERT S. OSBORN. Rochester, N. Y., The Lawyers' Co-operative Pub. Co., 1910. 501 p.

This is a comprehensive and excellent work on modes of testing handwriting and other topics therewith connected, describing how questioned documents of various classes have been cared for, the standard of comparison, photography, the microscope, alignment, pen position and pressure, writing instruments, variations in style, forgeries, disputed letters, ink, paper, folds, erasures, age of documents. The work contains over two hundred illustrations and is written mainly from the legal point of view without signs of much acquaintance, even in the brief bibliography appended, with the recent voluminous studies in this field.

Introduction to philosophy, by WILLIAM JERUSALEM. Authorized translation from the 4th edition by Charles F. Saunders. New York, Macmillan, 1910. 319 p.

This translation endeavors to help all who have a real interest in philosophy to an acquaintance with its language and its problems, and thus to stimulate independent reflection. The author's watchwords throughout have been "objectivity, perspicacity and brevity." The Germans have shown their appreciation by the fact that the book went through four editions in ten years. Its second aim is to examine the problems themselves and to make contributions toward their solution. The author's philosophy is characterized by the empirical view-point, the genetic method, and the biological and social mode of interpreting the human mind. He first treats preparation, principles, then criticises knowledge and epistemology, passes then to metaphysics and ontology, then to the methods of æsthetics, and finally to those of ethics and sociology.

Studies in the psychology of sex. Erotic symbolism; the mechanism of detumescence; the psychic state in pregnancy. By HAVELOCK ELLIS. Philadelphia, F. A. Davis Co., 1906. 285 p.

In this book the phenomena of the sexual processes are discussed before the attempt is made in the concluding volume to consider the bearings of the psychology of sex on social hygiene. Under erotic symbolism the author includes all the aberrations of the sex instinct although some have been deemed important enough for special volumes. Much stress is laid upon sexual equivalents. The mechanism of detumescence brings us to the final climax for which the earlier and more prolonged stage of tumescence is an elaborate preliminary. The art of love is that of preparation. The author, too, has treated at some length the psychic state of pregnancy, where the whole process in a sense reaches its goal. Woman in this state is "the everlasting miracle which all the romance of love and all the cunning devices of tumescence and detumescence have been invented to make more manifest." This is "the supreme position" which life has to offer and has its own problems. The book is full of suggestiveness.

Die Philosophie der Gegenwart; eine internationale Jahresübersicht. Herausgegeben von ARNOLD RUGE. Band 1 (Doppelband), Literatur 1908 und 1909. Heidelberg, Weisssche Universitätsbuchhandlung. 1910. 532 p.

This is an extremely serviceable book. It divides philosophical literature into 12 departments, viz., (1) journals, creative works and dictionaries; (2) texts, translations and critical works; (3) history of philosophy; (4) general philosophy; (5) logic and theory of knowledge; (6) moral, social and legal philosophy; (7) philosophy of history, language and culture; (8) natural philosophy; (9) philosophy of religion; (10) art; (11) psychology; (12) more popular works, aphorisms and essays. Two reflections are suggested here. One is that this division of subjects is far too elaborate. In looking for some special work or article, the writer of this notice had to look through several of these rubrics before finding what he wanted, but the chief criticism of the arrangement is that psychology is given so small a place and that so many works one would naturally expect to find under this caption are found under philosophy.

The Journal of Animal Behavior. New York, Henry Holt & Company. Vol. 1, No. 1, January-February, 1911.

This is a new journal in a new topic. The editorial board is composed of Madison Bentley, of Cornell University, Harvey A. Carr, of The University of Chicago, Samuel J. Holmes, of The University of Wisconsin, Herbert S. Jennings, of The Johns Hopkins University, Edward L. Thorndike, of Columbia University, Margaret F. Washburn, of Vassar College, John B. Watson, of The Johns Hopkins University, William M. Wheeler and Robert M. Yerkes, of Harvard University. It is published by Henry Holt and Company, of New York, and the first number contains 77 pages. The first article is an experimental study of the turtle, by D. B. Casteel. Then follow articles on The Reactions of Mosquitoes to Light in Different Periods of their Life History, by S. J. Holmes; A Study of Trial and Error Reactions in Mammals, by G. V. Hamilton; A Note on Learning in Paramecium, by Lucy M. Day and Madison Bentley; and a note by Robert M. Yerkes on Wheeler on Ants. Save the last, there is nothing approaching a book review. We are glad to know that the Journal is to have a book-review department.

Spiritism and Insanity, by MARCEL VIOLETT. Swan, Sonnenschein and Co., 1910. pp. 134.

In the presence of spiritistic facts men react diversely. Sceptics deny everything *en bloc;* serious savants endeavor to apply their scientific methods, but up to the present their efforts have remained barren; the rest are essentially believers. An imperious need inclines them to accord a supernatural origin to what they cannot understand naturally. Such a method is risky, but where is the science which risks nothing?

At a spiritistic meeting the air fairly vibrates with mystery, and all believe; but when they leave the seance most become preoccupied with everyday affairs, and the belief has little practical importance. Far otherwise is the case with certain ones of instable mental equilibrium. Here we find those of congenitally weak intelligence, for whom life is at best difficult, who seek consolation in spiritism, and find there only new tortures. Here, too, are the paranoiac temperaments, those suspicious of others, inclined to beliefs in persecution, whose weariness of life leads them to spiritism. Here are the self distrustful and melancholiac, and especially the hysterical and neuropathic, who tend to become subjects. Here some bring actual insanity, senile decay, minds weakened by excesses, all of which give the best of soils for spiritism to grow in.

For the very core of spiritism is the mystery of its facts; not what the facts are, but that they seem to be without natural cause. But this is closely analogous to hallucination. Both appear abruptly, without transition, without progression, preparation or natural explanation, and as the hallucination tends to produce automatism in its subject, so does the spiritistic phenomenon produce it in the sitters, already predisposed, selected in many cases as we have seen, from the instable.

To the spiritist, evoking a spirit means to bring back the perispirit, which is the mean between body and soul and the Intermediary between us and the invisible volitions about us. Th eidea of this constant *entourage*, the uncertainty as to the power of these spirits, of their intentions towards us, of our possible displeasing of them, of our weakness and defenselessness against them—all this is hypothesis, but hypothesis which offers no barriers, which can never be refuted, and which opens to infinite other hypotheses the more it is considered. Its guarantees are the senses of the spectators and their unanimous consent to the dogma and doctrines. But it may be compared to delirium in these respects: it originates, like delirium, in a miraculous fact, and the consequences drawn from this fact are purely hypothetical. Whether it be actual delirium or not, it constitutes a vast culture infusion for all errors, disequilibrations and madness.

So we get two classes of spiritistic insanities: 1, those evolving among the predisposed whose attack is referable to spiritistic preoccupations; and, 2, those who would have gone insane in any case, but to whom spiritism has given its own coloring.

Under the first come those exterior mediumnopaths, who are tormented by wicked spirits outside themselves; interior mediumnopaths, when the demon has taken possession. This possession may vary greatly in degree, the torments of the victim increasing until complete possession is attained, when depersonalization is complete, the delirium of greatness sets in, and the case passes into mediumnomania. The patient now considers himself a medium and is glad to be one, the spirit praises him, he has dreams of establishing a new religion, etc. All sorts of hallucinations may develop, and in the extreme stages the person loses all memory of his former self, perhaps even the notion of the existence of his body, becomes immovable and silent, but shows by the happy and calm expression, the ecstasy at which we can only guess. But some always remain melancholic.

In all these cases there are hallucinations, but in others there are none. Such cases rest their beliefs on illusions and delirious interpretations, especially the latter, and will evolve, according to temperament, towards melancholia, the delirium of persecution or of greatness. The last two are usually combined, the intelligence is intact on other points, and so the patient systematizes his delusion with much subtlety. His delusion becomes his curse, he is the persecuted victim, and he must defend himself by all means, often, in the end, by physical violence or even murder.

Spiritism may give a coloring to dementia præcox in its various forms, to general paralysis, to senile dementia, but these diseases would have evolved in any case.

In view of these facts a word of warning should be spoken, especially to the spiritists themselves: Sift your seances. Keep out the degenerate and unbalanced, and thus spare them possible madness and spiritism the discredit, danger, and fraud involved in having them for supporters.